



The Times of Their Lives

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TIMES *of* THEIR
LIVES

*Hunting History in the Archaeology
— of Neolithic Europe —*

ALASDAIR WHITTLE

THE TIMES OF THEIR LIVES

Hunting history in the archaeology of Neolithic Europe

Alasdair Whittle

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Front cover: Excavations by Miloje Vasić in 1933 at the great tell of Vinča-Belo Brdo, Serbia. Photo: Faculty of Philosophy, Belgrade University.

For Clare, Anna and Natalie

Contents

Acknowledgements

1. Hunting history
2. Long pasts, brief lives: the lenses of history
3. The rags of time: building chronologies
4. ‘Sometimes sudden and sometimes slow’: the reach of precision
5. Time lords? Community and leadership across the Neolithic sequence
6. An old Europe in a new light
7. Kinds of history: the future of the Neolithic past

Bibliography

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Chapter 1

Hunting history

The importance of history

If history requires narrative, it matters who tells the stories, and how. Several significant discussions of this, from Eric Wolf's *Europe and the people without history* (1982), Michel-Rolph Trouillot's *Silencing the past: power and the production of history* (1995) and Edward Said's *Orientalism* (1995), to Jack Goody's *The theft of history* (2006), agree that when people are denied their history, they can be rendered powerless. 'History reveals itself only through the production of specific narratives', as Trouillot puts it (1995, 25), and control of sources, archives, narrative and reflection is paramount (Trouillot 1995, 26). The theme has been continued in a recent collection of archaeological and anthropological papers on early Africa (Schmidt and Mrozowski 2014), whose title, *The death of prehistory*, also neatly evokes the aim of my book. In being habituated to write about the past, 'prehistoric' people of Europe and elsewhere in a very generalised, and frequently timeless, kind of way, I will argue that archaeologists have imposed a similar kind of denial of history, often unthinkingly. The aim of my book is to show how, using case studies from the European Neolithic period, we should try to write much more detailed narratives about people in the remote past of several thousand years ago. In so doing, we can attempt to take the pre- out of prehistory, and restore to centre-stage a sense of past people's actions, choices and decisions.

My reaction to the dominant, generalising and rather distancing kind of writing in archaeology comes because for most regions and for most sequences around the world, prehistorians have until recently only been able to assign the past people whom they study to more or less imprecise times (Bayliss *et al.* 2016). Our less than perfect chronologies come from the way in which time is measured and controlled (or thought to be), normally through an uneven combination of site stratigraphies, ordering of the material through typology and in some regional traditions through seriation, and radiocarbon dating. The default, conventional approach to the use and

interpretation of radiocarbon dates has essentially relied on just looking at them: the visual inspection of calibrated dates, or ‘eyeballing’. A radiocarbon sample from a few thousand years ago will calibrate to a date spanning 100–200 years (at two standard deviations); a group of such samples will not produce identical calibrated dates, even when they derive from the same event (the demise of a tree, the killing of a cow, or the death of a person) and eyeballing a graph of such dates has tended to include the extremes of the timespan indicated (Bayliss *et al.* 2016). Prehistorians and other archaeologists regularly exaggerate the duration of a given phenomenon as well as accepting the relative imprecision of its dating; without formal constraint, things will often appear to have started earlier, lasted longer, and ended later than was the case in reality (Bayliss *et al.* 2007). By way of contrast, dendrochronology can provide dates precise to a calendar year and even to a season within a given year, but in the European context, the waterlogged conditions in which wood is preserved are largely confined to the Neolithic and Bronze Age settlements on the fringes of the Alps – the Alpine foreland (Menotti 2004). These results are hugely important, as I will explore in several chapters in this book, but while they are very significant in helping to underpin and provide cross-checks for the more traditionally constructed chronologies in regions around them, they have not so far had much impact on the kinds of narratives in general which we choose to write about Neolithic Europe. (I should make it clear from the outset that my use of the label ‘Neolithic Europe’ includes what many scholars have called the Copper Age; I will discuss this later on.)

The best way out of this situation, it is now clear, lies in the rigorous application of Bayesian chronological frameworks for the interpretation of radiocarbon dates, in combination with the highest standards in sample selection and evaluation, critical examination of context and stratigraphy, and refinement of the ordering of associated material through typology and where possible seriation. The Bayesian approach, as I will set out briefly but in more detail in [Chapter 3](#), combines the distributions of probability which a group of radiocarbon dates represents with existing knowledge, for example of site stratigraphy, contexts, association and sample taphonomy (things which, to their credit, archaeologists are often very good at), to produce revised date estimates; since the inevitable scatter of radiocarbon dates around a given phenomenon has been formally constrained, these revised date estimates are often much more precise than the initial probability distributions. If they do the right things –

which they frequently fail to do – archaeologists in general and prehistorians in particular do not have to confine themselves any longer to the long term, and an imprecisely estimated long term at that, which has often been seen as the defining currency of the discipline. So this book has two goals. I want to explore the challenges of creating much more detailed sequences for the long span of the Neolithic in Europe, and I want to think about the consequences of finer-resolution narratives for our understanding of the lives of past people. Alongside the long term, or perhaps better, woven into it, we can think of a spectrum of histories at varying temporal scales, from the enduring grasp of traditions or the reach of social memory, covering one, two or more centuries, down to lifetimes and generations, measurable in decades. Whether we can get to even more precise chronologies, to eventful horizons and even events, is open to debate, a question which will surface several times in the chapters that follow.

Much is therefore open to change in the way we can write about the archaeological past, and how we can situate archaeological narrative. I believe that the consequences are potentially revolutionary, for practically every regional sequence across Neolithic Europe as a whole. Perhaps it helps to put this bold claim in context by reflecting on the development of my own involvement in the application of Bayesian chronological modelling.

Histories of the Dead, and Gathering Time

Looking back at my earlier work, I was always interested in the challenge of creating chronologies and getting things in the right order (Whittle 1985; 1988, [chapter 2](#)). When I excavated the causewayed enclosure of Windmill Hill, Wiltshire, in southern England, in 1988, however, the samples which I submitted for radiocarbon dating (Whittle 1993; Whittle *et al.* 1999) left much to be desired by modern standards, since the disarticulated bones in question could have been residual, and therefore older than the contexts in which they were deposited or otherwise ended up. By around 2000, I was working with Michael Wysocki on a project on the human bone from early Neolithic contexts in southern Britain, principally long barrows and long cairns. We had been struggling to win funding for radiocarbon dating, but were put in touch with Alex Bayliss, then of English Heritage (now Historic England), with whose support we were able to obtain a large set of dates – some 160 – for five long barrows. It was not the numbers of dates that changed everything, however, but rather their inclusion in rigorously constructed Bayesian models. The method had been promoted not

long before, including by Caitlin Buck who had worked briefly in our Cardiff department (Buck *et al.* 1996), and one of the first major applications had already been made, to the chronology of Stonehenge (Bayliss *et al.* 1997). Here was a remarkable set of results (Bayliss and Whittle 2007). Rather than belonging to an ill-defined early Neolithic of several centuries' duration, none of the long barrows predated the 38th century cal BC, and the main use of four of the five long barrows ended in the latter part of the 37th century cal BC. That was clearly not the whole span of such constructions, since the first phase of Wayland's Smithy probably started just after 3600 cal BC, with its small mound probably constructed in the late 36th to early 35th century cal BC, and its second phase, with an architecture remarkably similar to that of 37th-century West Kennet, in the middle to later part of the 35th century cal BC. The first phase of depositions of human remains at Wayland's Smithy was probably of very short duration, and three of the other four long barrows were, according to the models, in primary use for only one to three generations (taking 25 years as a generation, discussed in more detail in [Chapter 2](#)), with Ascott-under-Wychwood the longest-lasting, probably over three to five generations (Bayliss and Whittle 2007). Suddenly we were talking not just about centuries but parts of centuries and even decades. We were being offered a first glimpse from this obviously small sample of how things might have their own very specific times. At a period in British Neolithic studies at least when two of the main characteristics of 'monuments', including long barrows, were seen to be their drawn-out processes of construction and their subsequent ability to endure for long periods of time, we had been given a revealing insight into how things could have happened much more quickly than we had previously imagined possible.

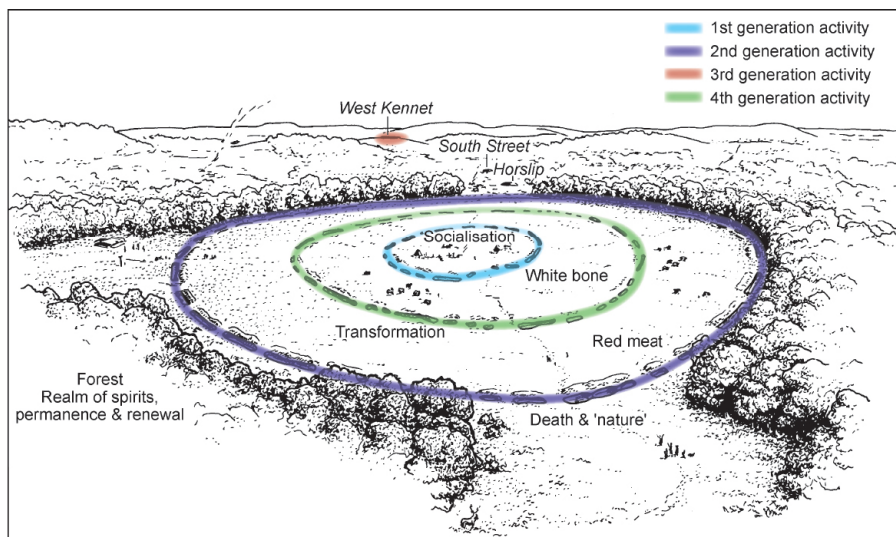


Fig. 1.1: The sequence of construction at the Windmill Hill causewayed enclosure and West Kennet long barrow in the 37th century cal BC. After Whittle et al. (2011). Background image by kind permission of Josh Pollard.

The surprises continued. By 2003, Alex and I, joined by Frances Healy, who had been working on the publication of Hambledon Hill with its excavator Roger Mercer, were lucky enough to gain major funding for a much more ambitious programme of dating of the early Neolithic causewayed enclosures of southern Britain and Ireland.¹ Mainly distributed across southern Britain, these sites had long attracted attention and excavations had often produced a wealth of material from their ditches, much of it suitable for dating. Some claims had been made for very early dating within the British Neolithic (at the time that meant around or soon after 4000 cal BC), but many researchers were probably again content to assign them to the broad span of several centuries which defined the British early Neolithic, though there were some perceptive suggestions of a more specific date within that span (Cleal 2004; Bradley 2007). By combining in our many models over 400 new results from carefully selected samples with over 400 existing radiocarbon dates from nearly 40 such enclosures, we were able to show that causewayed enclosures were probably first constructed in the last decades of the 38th century cal BC, just before 3700 cal BC, and probably appeared from east to west across southern Britain, to reach a peak in numbers in the latter part of the 37th century cal BC; after a lull, new constructions resumed in the early 36th century cal BC, to fall right away by the middle of that century (Whittle *et al.* 2011).

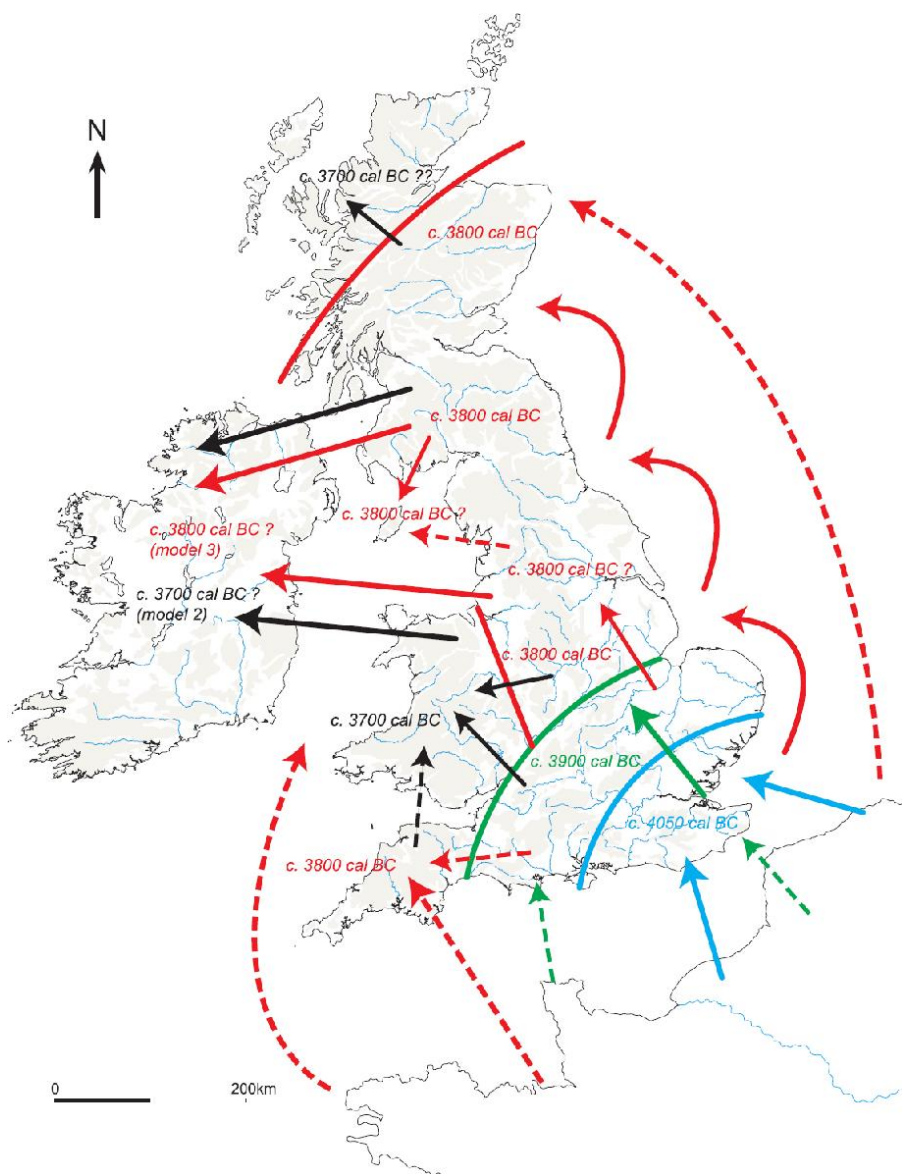


Fig. 1.2: Model for the spread of Neolithic things and practices at the start of the Neolithic in Britain and Ireland. After Whittle *et al.* (2011).

This much bigger set of results could be joined to those for long barrows and other kinds of monument. It now looked as though causewayed enclosures first appeared some time after long barrows, perhaps getting on for a century later, or, judging by the early dating of the Coldrum monument in Kent (Whittle *et al.* 2011, chapters 7 and 14; Wysocki *et al.* 2013) substantially later; the demise of four of our sample of five well dated long barrows coincided with the end of the first surge in enclosure construction, in the decades around 3625 cal

BC; and the final ending of new enclosure constructions appeared to coincide with the emergence of the first, very different cursus monuments. Duration again came to the fore as an issue of key significance. Although the primary use of some enclosures lasted for some three centuries, as had long been suspected on the basis of ditch stratigraphies and the material they contained – Hambledon Hill, Dorset, had already been shown to be an example of this durability (Mercer and Healy 2008) – others proved to be in shorter use, for not much more than a century, and some to have been probably very short-lived indeed. Construction again appeared to be a much swifter affair than previously imagined, and the examples of the three circuits of the Windmill Hill enclosure and the West Kennet long barrow were shown to be successive and inter-woven over several generations in the middle part of the 37th century cal BC (Fig. 1.1).

By modelling some 1400 existing dates for early Neolithic contexts across southern Britain, Ireland and Scotland south of the Great Glen (see also Griffiths 2014), we were also able to put long barrows and enclosures into a much wider and more secure (though still of course provisional) context. Debate on the nature of the Mesolithic-Neolithic transition had been (and continues to be) intense, but most protagonists, whether supporters of colonisation or indigenous transformation, had been using an informal estimate of around 4000 cal BC as the horizon of change. Our models suggested a very different picture, with the first Neolithic ‘things and practices’ (as we called it) probably appearing in the area of the Greater Thames estuary in southeast England in the 41st century cal BC, to spread again east to west, gradually at first and accelerating with time, to encompass most of Britain and Ireland by the 38th century cal BC (Fig. 1.2). Although the modelling did throw up one major problem, the potentially early dating of the enclosure of Magheraboy just outside Sligo in western Ireland, overall a coherent sequence emerged: of early, small-scale beginnings in the south-east, an accelerating spread to the north and west, and the successive introductions of places and constructions for collective burial and commemoration, and then larger-scale assembly. With better (though for barrows and the wider early Neolithic context, still far from perfect) timing, also came a sense of tempo, and the pace and nature of change.

Old questions could now be seen in a new light. Perhaps we could relate beginnings in the south-east to the offshoot of major changes in settlement on the immediately adjacent continent; perhaps initial small-scale colonisation was combined with subsequent transformation of indigenous communities in Britain; perhaps Ireland

could be seen as inextricably enmeshed in this process; and perhaps the increasing tempo of change requires a competitive dynamic in social relations (Whittle *et al.* 2011, chapter 15; see [Chapter 5](#) for further discussion). Why, although we know for sure that the building of very similar enclosures goes back before 4000 cal BC on the adjacent continent, did it take some three centuries of development before people chose to replicate the idea in southern Britain? Whatever the answers, we were now asking much more specific questions of a much more detailed sequence. From that process, I argue, we were creating narrative, full of potential links, motives, agency and plot (Ricoeur 1980; 1984). Whatever the many remaining uncertainties, I believe that we were beginning to write a much more specific kind of history than previously possible, if history is defined as the telling of true stories, the interpretation of facts, and the making of ‘an interesting, coherent and useful narrative about the past’ (Arnold 2000, 13).

The Times of Their Lives

The next step was a successful application to the European Research Council for a major project, *The Times of Their Lives*, to carry out a series of Bayesian case studies across a large swathe of Neolithic Europe. Our aims included showing how formal modelling of radiocarbon dates could help to refine even further the seemingly sturdy chronological framework created by patient, detailed studies of the material going back to the late 19th century. We wanted to explore the application of the Bayesian approach to a much more varied set of situations and challenges than was possible or necessary in our work in Britain and Ireland, and to engage with important themes in the European Neolithic, including settlement, monumentality and materiality, from the sixth to the third millennia cal BC. This was therefore not intended to be a single, more or less geographically coherent regional study of the kind we had carried out in southern Britain and beyond, but a series of explorations designed to demonstrate best practice, test ideas and provoke reactions. There are very encouraging signs that the Bayesian approach is increasingly being adopted (as discussed further in [Chapter 3](#)), but plenty of sceptics remain.

I will draw on our case studies from *The Times of Their Lives* (ToTL) extensively and in detail in [Chapters 4](#) and [5](#). Just three selected highlights will give an initial idea of their scope, and they serve further to show that we can create much more detailed narratives virtually everywhere, by the rigorous application of the formal chronological approach. Our ToTL applications have been a mixture of

site-based investigations and regional studies, but imagine what even richer histories could be written when neighbouring sites have been dated with equal precision, and when wider regional coverage has been accomplished.



Fig. 1.3: Scenes from an excavation: the Vasić fieldwork at Vinča-Belo Brdo. Photos: Faculty of Philosophy, Belgrade University.

One of the major features of Neolithic and Copper Age archaeology in south-east Europe are the tells or settlement mounds: impressive accumulations on the same spot of the remains of successive occupations, including the debris from houses. We have studied two of these, Vinča-Belo Brdo near Belgrade, Serbia (Tasić *et al.* 2015; 2016a; 2016b), working with Nenad Tasić, Miroslav Marić and colleagues, and Uivar, near Timișoara in western Romania, working with Florin Drașovean, Wolfram Schier and colleagues (Drașovean *et al.* 2017). Tells have been the focus of intense study for over a century; the excavations at Vinča-Belo Brdo began in 1908 (Fig. 1.3). Their deep stratigraphies and the abundant and varied material found in them, but especially the pottery, have long been the means to create increasingly detailed chronologies, both on a site by site basis and within and between regions. One magisterial study, by Hermann Parzinger (1993, 273), created a detailed chronology in a series of horizons reaching from the northern Balkans to southwest Asia,

deliberately without the aid of any radiocarbon dating at all. In many other cases, however, radiocarbon dates have been obtained, though often using samples of less than ideal quality. For both Vinča-Belo Brdo and Uivar we were able to greatly increase the numbers of samples and be much more selective in their choice. At Vinča-Belo Brdo, the highest tell in the central Balkans, an interwoven, three-stranded approach, using the whole sequence from the first excavations, the top of the mound from more recent investigations, and a new, though spatially very limited, profile through most of the tell, has given the first, complete, detailed history of the mound. It began probably just after 5300 cal BC, and continued to rise seemingly inexorably until the mid-46th century cal BC, ending probably in the 4540s or 4530s (Tasić *et al.* 2015; 2016a; 2016b).

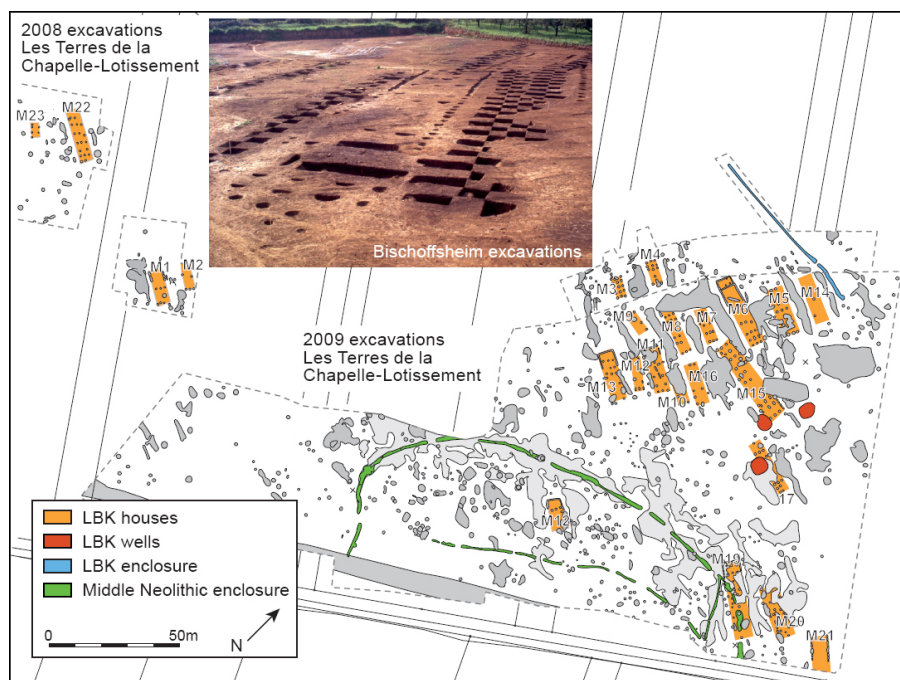


Fig. 1.4: LBK archaeology in Lower Alsace. Inset photo: longhouses under excavation at Bischoffsheim (Philippe Lefranc); plan: principal features at Entzheim (Philippe Lefranc).

Here indeed is the Neolithic long term: century after century of it. For the first time, however, in the new profile at Belo Brdo, unburnt as well as burnt houses could be properly documented, and an intriguing pattern is apparent of burnt houses in the lower and upper parts of the sequence, sandwiching unburnt houses in the middle part. The successive houses had varying durations, variously from one or two to several generations (Tasić *et al.* 2015; 2016b); comparable figures

come from Uivar (Draşovean *et al.* 2017; Schier *et al.* forthcoming; see further detail in [Chapters 4 and 6](#)). The last two horizons dated from the Belo Brdo excavations going back to the 1970s also consisted of rows of burnt houses; there was an interval of probably 25 years between these horizons, and one of the structures in the uppermost horizon probably lasted for as little as 15 years (Tasić *et al.* 2015). The more precise dating now available also enables modelled estimates to be given for the changing pottery and other material forms through the sequence of the tell, and we have been able to compare these with the existing corpus of radiocarbon dates for the wider distribution of the Vinča culture or network across the northern and central Balkans (Tasić *et al.* 2016a; Whittle *et al.* 2016). As we will see in much more detail in [Chapters 4 and 5](#), there have been long-running debates about the nature of the Vinča network as a whole, the role of tells in regional patterns of settlement, the relationship between households and communities, the scale of house burnings and the motives for them, and the conditions in which tells ceased to accumulate and the network as a whole came to an end. Our much more precise and extensive date estimates serve to energise these discussions, showing once again how we can hope to create much more detailed sequences, and from those write more complex narratives.

In Lower Alsace, in the upper Rhine valley, we teamed up with Philippe Lefranc and Anthony Denaire, to study the cultural sequence from the later sixth to the late fifth millennium cal BC (Denaire *et al.* 2017). Both Philippe and Anthony had written doctoral theses on pottery, respectively of the early Neolithic or LBK, and the successive middle Neolithic groups (Lefranc 2007; Denaire 2009). Each had created a seriation of the decorated pottery, through correspondence analysis; where those ran out towards the end of the middle Neolithic sequence, the gap was plugged by typology. This framework thus enabled us to date the sequence by taking samples from each successive phase suggested by the seriations. A general pattern of style change every two to three or three to four generations was found. This timescale also revealed a subtle pattern of shifts in the numbers and density of known settlements, suggesting a world that on the ground was far from static, but rather subject to endless adjustment, and continual comings and goings ([Fig. 1.4](#)). So here once more is the Neolithic long term, but again far from unchanging in detail. The major surprise of this study was a hiatus between the early and middle Neolithic. In terms of the conventional chest-of-drawers chronologies for Lower Alsace, LBK V is succeeded by the Hinkelstein group, and in other regions roundabout, late LBK is succeeded by the

Stichbandkeramik culture. Now although we could not date LBK V in Lower Alsace itself, we were able to model comparable late LBK dates from neighbouring regions, to the period around or just after 5000 cal BC (the detail is set out in [Chapter 4](#)), whereas within Lower Alsace the Hinkelstein group did not appear to be earlier than the first half of the 48th century cal BC. The strong probability of a gap was reinforced by modelling existing dates (again a far from perfect set of samples) for other parts of the Rhine valley. In Lower Alsace, the landscape did not appear to be reoccupied on the same sort of scale as witnessed in the LBK until the Grossgartach phase, in the late 48th and earlier 47th century cal BC. Now there have been many suggestions of a troubled, perhaps in places very violent, end to the LBK in parts of western and central Europe, but it has normally been assumed that there was continuity of settlement after this in most regions. Our revised chronology does not of itself offer an explanation for hiatus, but it opens up possibilities for discussion and future research. Was the LBK world knocked off course by internal conflict, over-population or climatic deterioration, or should we think harder about the impact of disease, or all of these possible factors in combination? And there are other, wider implications too. If one detailed study of this kind has thrown up the strong probability of significant hiatus, how many others may lurk in the seemingly robust chronological framework for Neolithic Europe as a whole? By repetition and assumption, we have created a Neolithic world dominated by slow change and continuity. What if a new history proves to have a much faster pace and many more gaps and abrupt changes?

We worked on the very big Copper Age site at Valencina de la Concepción, near Sevilla in south-west Spain, with Leonardo García Sanjuán and many other colleagues (García Sanjuán *et al.* 2017; 2018). Broadly speaking, this belongs to the late fourth to the middle of the third millennium cal BC. Here is a complex probably more than 450 ha in extent, comprising a wealth of pits and other subsoil features, working surfaces, stretches of large ditches, and megalithic tombs (García Sanjuán *et al.* 2013a). There are, however, no obvious houses or dwellings, and very few, highly fragmented querns have been found, though pits contain a wide range of material, and it seems clear that both ivory and copper were worked here. Pits also frequently contain burials, and deeper pits or ‘artificial caves’ can have stratified, successive burials. The megalithic tombs take the form of tholoi, or circular mounds with a passage leading to one or two more or less central chambers. Very large tholoi are known, such as

the famous examples of La Pastora and Matarrubilla, discovered in the mid-19th and earlier 20th centuries respectively, or the more recently excavated Montelirio, whose mound was 75 m in diameter, with a passage nearly 40 m long leading to a main and a subsidiary chambers (Fernández Flores and Aycart Luengo 2013; Fernández Flores *et al.* 2016). Without the scale of disturbance suffered by the others mentioned, Montelirio showed a collective deposit of some 20 individuals, 12 of them female. These were clothed or covered by unusual bead-covered costumes (Figs 1.5–1.6; and see Figs 5.12–5.16). Another substantial tholos, 10.042–10.049, from the adjacent PP4-Montelirio sector, also had spectacular finds (García Sanjuán *et al.* 2013b; Morgado *et al.* 2016). It had previously been supposed that a domestic or residential area could be distinguished from a funerary area, since the notable tombs seem to group in the more easterly parts of the complex, but this distinction is now less clear. The site can be aligned with other major complexes of the Copper Age in southern Iberia, where an intense debate has focused on the degree of social differentiation which might have emerged by this date (Cruz Berrocal *et al.* 2013: and see Chapter 5). It has even been proposed that Valencina was the centre of some kind of early state, with production of copper on a practically industrial scale, with attendant environmental damage (Nocete Calvo *et al.* 2008).



Fig. 1.5: Individual UE 343 from the main chamber of the Montelirio tholos at Valencina de la Concepción. After Fernández Flores *et al.* (2016).

We were able to model some 130 radiocarbon dates. Results were far from perfect, as numerous samples failed – typically enough for this kind of hot, dry environment – because of poor collagen preservation, and the sequence which is emerging can certainly be improved in the future. It looks, however, as though the Early Copper Age sequence at Valencina had probably begun in the 32nd century cal BC (García Sanjuán *et al.* 2018). There is a good case for seeing the construction of tholoi, the lavish funerals which accompanied at least some of them, and the very wide connections achieved by some individuals or groups, witnessed in the acquisition of ivory from Africa and the Near East (García Sanjuán *et al.* 2013b; Nocete Calvo *et al.* 2013), as probably confined at Valencina to the 29th and part of the 28th centuries cal BC. These may evoke emergent but unstable elites, with limited capacity to break up traditional Neolithic bonds of collectiveness and communality (R. Chapman 2008, 243; Díaz-del-Río 2011; García Sanjuán and Murillo-Barroso 2013, 133–5). There is a plausible case for seeing the very biggest tholoi, such as La Pastora and Matarrubilla, as belonging to a period of transition to new practices in the middle of the third millennium cal BC. Though this site sequence could still be more precisely defined, the new chronology helps, crucially, to suggest a much more dynamic and unstable set of developments than offered in generalised, broad, informal estimates of date.



Fig. 1.6: Detail of the costumed body of individual UE 343 in the main chamber of the Montelirio tholos at Valencina de la Concepción. After Fernández Flores et al. (2016).

From chronicle to narrative

These and the other examples which I will explore in the following chapters are the basis for claiming that we can achieve chronologically much more precise accounts of the past, routinely across a broad range of contexts and situations. This does not do away with the long term; rather, we can time the long term much better, and unpick its constituent elements, rhythms and tempo. If we can get at different timescales, and if things look different when examined at different timescales, we have to think much harder about how best to combine them. Our new-found ability to construct much more detailed sequences should now illuminate the more familiar and traditional concerns of the discipline of archaeology with the broad sweep and the long term. From such detailed chronologies, we can create narratives rich in connections, agency and plot, and on that basis, we can align this kind of archaeology, even of a remote past several thousand years old and lacking written records, much more closely with the general focus of historians in general.

Not everyone would agree. Many prehistorians are still uncomfortable with the choice of narratives now available, or have not yet critically rethought old habits of interpretation. Others again

are seeking to shift the nature of interpretation in a direction which I see as opposed to a more historical approach. It is worth briefly listing some of the points of view that need to be challenged, if the approach which I am advocating is to be widely adopted.

There are probably very few if any researchers who remain sceptical about radiocarbon dating in general, nearly 70 years after its appearance. But because things can go wrong – which they undoubtedly do from time to time – the response can sometimes be that certain kinds of samples are suspect. In [Chapter 3](#), I will discuss both routine risks, and the need for rigour in the application of formal chronological modelling (Bayliss 2015; Buck and Meson 2015; Buck and Juarez 2017; Pettitt and Zilhão 2015). If the overwhelming majority of researchers accept and use radiocarbon dating in their work, many still exploit it uncritically. This takes several forms. Despite the clear guidance given by Hans Waterbolk (1971) nearly 50 years ago, not everyone submits appropriate samples, without significant age-offset. A great many accounts still eyeball radiocarbon results, which as already noted routinely gives us the wrong or imprecise answers. Summing of radiocarbon results is very popular, as it is relatively quick and easy to operate, and enables big datasets to be employed. In European Neolithic studies this practice goes back quite a long way, first using uncalibrated (Ottaway 1973; Geyh and de Maret 1982; Breunig 1987), and then calibrated (Aitchison *et al.* 1991) radiocarbon dates. Prominent examples at the present time include applications in the fields of demography and cultural change (Shennan *et al.* 2013; Manning *et al.* 2014). This method tends to produce inaccurate chronologies of exaggerated duration, because it smears results without precision, and tends to reproduce the shape of the calibration curve (Bayliss *et al.* 2007, 9–11; Chiverrell *et al.* 2011; Contreras and Meadows 2014; Bishop 2015); individual results are not critically scrutinised, on the grounds that pattern will emerge robustly from the quantity of data employed (Manning *et al.* 2014, 1065–70).

In some quarters, especially in British Neolithic studies, much more could be done with both typology and seriation of material. One cross-period study shows how much can be inferred about the pace of cultural change by looking at the spans of suggested phasing in the material, even if the basis for the durations is left somewhat unclear (Siegmund 2012). Nonetheless, we have to remember that such orderings of the material do not inevitably yield robust chronology, and there is a tendency in many studies right across the continent to employ typological schemes as a substitute for robust chronology. Because this is Type C, say, the argument goes, the date (very often an

eyeballed, rounded date anyway) must be 5000 cal BC, because that is how other examples have been interpreted.

All these bad habits are correctable by the adoption of best practice. There is another set of objections which rest more fundamentally in differences of approach to the interpretation of the archaeological record, and such a clash of philosophies is less easily resolved. One example is the recent discussion by Gavin Lucas (2015; see also Lucas 2012) of contemporaneity and synchronism. While advances in the application of Bayesian modelling are acknowledged (Lucas 2015, 2), it is argued that the precision currently achievable is not sufficient to determine exact synchronism (Lucas 2015, 3–4). The argument then shifts to typology and seriation, in which the focus is on objects; ‘types have to overlap in order to draw out a sequence. This is an explicitly non-synchronous idea of contemporaneity’ (Lucas 2015, 4). In this view, contemporaneity becomes a ‘relation between objects’, and issues of dating and chronological resolution ‘become relative to our interpretation of objects, rather than vice versa’ (Lucas 2015, 6–7). It is revealing that rather than questioning the limits of chronological precision currently available or conceivable in the future, or dealing with probability, the way ahead is seen to lie with objects. This position in turn relates to the much greater attention given in recent theorising to objects: the material or ontological turn (Lucas 2012, 157; see also Harris and Cipolla 2017), in which agency and affect are seen as much more widely distributed and not just in the hands of humans.

Another recent, bold claim is for ‘process archaeology’ (Gosden and Malafouris 2015). This is based on three very general postulates: that reality consists of modes of becoming, everything in the universe is in motion, and the universe and humans are in a permanent state of flow, rearranged into temporary form (Gosden and Malafouris 2015, 702–3). These high-level starting points might be disputed in detail, but what are relevant here are the six accompanying temporal propositions, from the shortest to the longest timescale (Gosden and Malafouris 2015, 704–12). The six temporal propositions of process archaeology range from the creation of form (for example in the emergence of a pot in the potter’s hands) to the long-term history of organisms, materials and ecosystems. The emphasis throughout is in on flow and contingency; ‘human history is made up of streams of material and streams of people’ (Gosden and Malafouris 2015, 711) and ‘there is no direction to human history ... human history, long or short, has no overall direction’ (Gosden and Malafouris 2015, 712). Some of these propositions might be accepted, but others – for

example the claim about contingency – deserve much further debate. What is revealing again is the complete absence of discussion of how we are to measure flow and motion; the messy business and hard work of creating precise chronologies as the basis for detailed narratives are out of sight in this standpoint.

Finally, there are those who emphasise the fragmentary nature of the archaeological record. Fragmented evidence need not be seen as confined to archaeology; the nature of sources and archives in medieval history is a case in point (Arnold 2008, [chapter 2](#)). For some archaeologists, this remains compatible with the telling of true stories. Ruth Tringham (2015), for example, has pursued the idea of ‘recombinant histories’, using diverse fragments of evidence from Çatalhöyük. But for others, such as Bjørnar Olsen (2012; see also Olsen 2010), the very nature of fragments, mixings and compressions precludes the construction of coherent narratives at all. In this view – diametrically opposed to what I advocate in this book – we should simply be content to celebrate ‘an archaeology that sacrifices historical narratives in favour of a trust in its own ruined things, things that emerge from and bring forth a different past’ (Olsen 2012, 25). Allied to this is the view that archaeological evidence exists only in the present, and should therefore be a study of ‘gaps, absences and missing elements’, using a ‘principle of reiteration’ rather than a principle of sequence, because the ‘time of a pure, linear, chronological sequence of events’ is seen as obsolete (Olivier 2015, 85). This is again connected to a view of things; ‘whereas history seeks to establish what happened to people, archaeology explains what happens to things, in other words, to the material world of human communities’ (Olivier 2015, 189; see also Witmore 2014).



Fig. 1.7: Simplified location map showing the principal sites discussed or mentioned in this book. Full site names are given in the text. Some regions are also indicated. 1 Links of Noltland; 2 Pool; 3 Midhowe; 4 Braes of Ha'Breck; 5 Quanterness; 6 Maeshowe; 7 Stones of Stenness; 8 Barnhouse; 9 Ness of Brodgar; 10 Ring of Brodgar; 11 Bookan; 12 Skara Brae; 13 Achnacreebeag; 14 Ballynahatty; 15 Magheraboy; 16 Carrowmore; 17 Carrowkeel; 18 Poul nabrone; 19 Knowth; 20 Newgrange; 21 Fornham All Saints 22 Coldrum; 23 Wayland's Smithy; 24 West Kennet; 25 Windmill Hill; 26 Avebury; 27 Silbury Hill; 28 Fussell's Lodge; 29 Larkhill; 30 Ascott-under-Wychwood; 31 Hazleton; 32 Wor Barrow; 33 Hambledon Hill; 34 Severn estuary footprints; 35 Gwernvale; 36 Garn Turne; 37 Bury; 38 Les Mournouards; 39 La Chaussée-Tirancourt; 40 Cuiry-lès-Chaudardes; 41 Bazoches; 42 Boury-en-Vexin; 43 Passy; 44 Balloy; 45 Mont St Michel; 46 Tumiac; 47 Er Hroek; 48 Prissè-la-Charrière; 49 Vendenheim; 50 Souffelweyersheim; 51 Ingenheim; 52 Bischoffsheim; 53 Entzheim; 54 Bernolsheim; 55 Obernai; 56 Oberschaeffolsheim; 57 Kolbsheim; 58 Chalain; 59 Clairvaux; 60 Château-Percin; 61 St-Michel-du-Touch; 62 Villeneuve-Tolosane; 63 La Draga; 64 Vila Nova de São Pedro; 65 Perdigões; 66 Leceia; 67 Zambujal; 68 Valencina de la Concepción; 69 Marroquies Bajos; 70 Camino de Las Yeseras; 71 Los Millares; 72 Elsloo; 73 Langweiler (Aldenhoven) 74 Erkelenz-Kückhoven; 75 Koslar; 76 Mayen; 77 Miel; 78 Urmitz; 79 Kapellenberg; 80 Wiesbaden-Schierstein; 81 Blatterhöhle; 82 Bruchsal; 83 Klingenberg; 84 Herxheim; 85 Kilianstädten; 86 Vaihingen; 87 Stuttgart-Mühlhausen; 88 Talheim; 89 Ehrenstein; 90 Bad Buchau-Torwiesen; 91 Sipplingen; 92 Pfyn; 93 Niederwil; 94 Hornstaad; 95 Arbon-Bleiche; 96 Zürich; 97 Sutz-Lattrigen; 98 Horgen; 99 Egozswil; 100 Burgäschisee; 101 Concise; 102 Ice Man; 103 Alvastra; 104 Liselund; 105 Büdelsdorf; 106 Flintbek; 107 Racot; 108 Olszanica; 109 Asparn; 110 Brunn; 111 Nitra; 112 Pityerdomb; 113 Lengyel; 114 Mórág; 115 Zengővárkony; 116 Alsónyék; 117 Szederkény; 118 Versend; 119 Csőszhalom; 120 Bojt; 121 Berettyóújfalu-Herpály; 122 Hodmezővásárhely; 123 Uivar; 124 Pařta; 125 Vinča; 126 Starčevo; 127 Stubline; 128 Tordoș; 129 Pločnik; 130 Divostin; 131 Okolište; 132 Pietrele; 133 Durankulak; 134 Varna; 135 Nebelivka; 136 Maidanetske; 137 Çatalhöyük.

My argument

In this book, I set out a very different kind of case (Fig. 1.7). I argue that we now have the means to build much more precise chronologies, and using these that we can move from creating chronicles to constructing interpretive narratives. Such narratives are the basis for a

kind of history. We can think of taking the pre- out of prehistory, even in the absence of written records. In my view, Neolithic (and Copper Age) histories work best when they centre on people's actions, choices and values. They are at their most illuminating at the scale of lifetimes and generations, but that in turn should be combined with other scales.

In [Chapter 2](#) I consider scales, and what is involved in writing different kinds of history. In [Chapter 3](#), I look at the means currently at our disposal for constructing robust chronologies, which will be the basis for narratives. I then set out and reflect on a much wider and more detailed set of case studies of what the sustained application of a formal chronological approach in a Bayesian statistical framework can bring to the study of the European Neolithic; that is the substance of my long [Chapter 4](#). Why does all this effort make a difference? I offer one sustained example in [Chapter 5](#), considering what more precise timescales offer for our understanding of processes of social differentiation through the millennia of the European Neolithic (and Copper Age). In [Chapter 6](#), I come back to the challenge of combining different timescales and discuss how much more precise date estimates affect our view of the long term. I suggest sets of contrasts between the flow and repetition of daily life everywhere, gradual alterations in subsistence practice and population levels, more dynamic social changes in many regional sequences, and a series of points across the Neolithic millennia when wider shifts, linkages and disruptions can begin to be synchronised. These are some of the building blocks of Neolithic history. That leads to a final brief consideration of the future of the Neolithic past, in [Chapter 7](#).

Note

- ¹ See Acknowledgements: in this case from English Heritage and the AHRC.

Chapter 2

Long pasts, brief lives: the lenses of history

The telescope and the microscope have played key roles in our modern understanding of the world. In the early 17th century Galileo Galilei developed the refractive telescope, invented by the Dutch, and with it made a series of observations of the Moon, the stars, Jupiter and the Sun, which served to confirm the Copernican system (du Sautoy 2016, 193; Graham-Smith 2016). For his efforts, he was rewarded with censure from the church. Later in the 17th century Robert Hooke developed the microscope, and used it to bring small things closer (Fig. 2.1). His *Micrographia*, published in 1665, included a famous image of the common flea, the previously unimaginable detail making considerable impact on his contemporaries; ‘the most ingenious book that ever I read in my life’, commented Samuel Pepys in his diary. Hooke had no trouble with the authorities, though he ended as a rather bitter rival of Newton (Pumphrey 1995; Inwood 2002).¹

Simplifying and cutting a long story short, telescopes can now see further and further into space, and high-resolution microscopes and other machines² enable the investigation of matter to the level of particles. They have been central to the emergence of the two central understandings of modern physics: the relativity of curved space-time, and the uncertainty of the activity and interaction of particles at a molecular level (Rovelli 2014). Classically, the General Theory of Relativity and quantum mechanics are not compatible, and no reconciliation, such as currently explored in loop quantum gravity, has yet been agreed (Rovelli 2014, 38, 40). In this understanding, things look different at different scales. Although self-similarity at different scales can be found in some aspects of the world, according to the fractal theory promoted by Benoît Mandelbrot (du Sautoy 2016, 60–1; cf. Gaddis 2002, 81–4; Harris and Robb 2013, 26), this does not apply at all levels. There are similar difficulties in the study of the past and the writing of history.



Fig. 2.1: Robert Hooke. By kind permission of Christ Church Library, Oxford.

Both historians and archaeologists work at many different scales, and fashions vary. The French *Annaliste* school, including in the prominent form of Ferdinand Braudel, advocated a range of scales, from the long term or *la longue durée*, to the medium term of social history or *conjonctures*, working over a span of decades and more, and events. In the introduction to his great *The Mediterranean and the Mediterranean world in the age of Philip II*, Braudel (1972, 21) used the metaphor of the sea itself, with *l'histoire événementielle* to be seen essentially as ‘surface disturbances, crests of foam that the tides of history carry on their strong backs’. Many historians now operate within what has been called the Short Past (Guldi and Armitage 2014), covering decades to a couple of centuries. Some concentrate on what has been called micro-history (Brewer 2010), at the scale of individuals and particular contexts. By way of contrast, many archaeologists have seen themselves as the guardians of the long term, or ‘deep history’ as it has been called more recently (Shryock and Smail 2011; Gamble 2013). That has not prevented laudable efforts to unpick the contents of deep history (Shryock and Smail 2011; Gamble 2014), but the chronological currency of many accounts, in a variety of interpretive styles, has been no more precise than centuries at a time. Those interested in events or eventful horizons are few and far

between (Bolender 2010), and others have pursued particular kinds of theme and context by dodging chronology altogether, placing individuals in a kind of timeless past.

The instruments of both historians and archaeologists are neither space telescopes nor high-powered electron microscopes, but the writing of narratives and the choice of scales (Arnold 2000, 13; Gaddis 2002, 96). To write about the long term may cut out the noise and concentrate on the signal,³ but at the risk of simplifying the story. To concentrate on the short term and particular contexts may get closer to individuals and particular contexts, but at the expense of perceiving a bigger picture. How long or short should our chosen timescales be, and indeed how wide or narrow should our spatial scales be? And can differing scales be combined, and if so how? In this chapter, I explore these difficult questions, arguing first that a much greater choice of scales exists than many ‘prehistorians’ have fully realised; and secondly, that if we can now routinely get to the scale of lifetimes and generations, we can begin to write much more detailed narratives, which we can call a kind of history. As another *Annaliste*, Lucien Febvre, put it, ‘the concept of prehistory is one of the most ridiculous that can be imagined’ (1973, 35) (Fig. 2.2).



Fig. 2.2: Top: Lucien Febvre; below: Fernand Braudel.

Practising scales

Time perspectivism

‘Time perspectivism’ has not generally caught on in archaeology (Murray 2008, 175), though it is commonplace in biology and other disciplines (Behrensmeyer and Schindel 1983; Lyman 2003). It represents the most explicit effort so far to worry about the choice of scales, and as such deserves careful consideration. Its leading advocate has been Geoff Bailey (1981; 1983; 2007; 2008). His basic argument is first, that ‘changes in the time scale at which we make observations change what we see’, and secondly, that the archaeological record is a palimpsest, of ‘variable but generally coarse resolution’ (G. Bailey 2008, 13); palimpsest is seen as ‘a superimposition of successive activities, the material traces of which are partially destroyed or

reworked' (G. Bailey 2007, 203). Following from this, although multi-scalar accounts can be envisaged (G. Bailey 2008, 24, 26), the possibility is asserted of seeing in the long term 'a pattern and an order of reality not available on shorter time scales' (G. Bailey 2008, 17); many archaeologists are seen as wanting to investigate individuals and to write narratives of lived, experienced time, but with data that are insufficient for the task (G. Bailey 2008, 22, 27). 'Moments in time', like the death of the Ice Man, are argued to be very rare (G. Bailey 2008, 22), and while the increased number of radiocarbon dates and improvements in dating in general are very briefly acknowledged, they are seen paradoxically as an obstacle, because of diverting 'attention away from the analysis of things in themselves and especially away from the different temporalities that are inherent in different sorts of palimpsests' (G. Bailey 2008, 25). It is worth quoting this view at greater length:

Improved chronologies are constantly being sought as the indispensable prerequisite to do something else. But what is that 'something else', and what degree of chronological resolution is required to study it? One suspects that what is being sought is the holy grail of a universal high-resolution chronology that will enable us to write about the history of the Lower Palaeolithic in the same detail as the history of the medieval period or the twentieth century, something that a time perspective view of the world suggests is a physical impossibility in the universe in which we happen to live (G. Bailey 2008, 25–6).

The reference to the Palaeolithic is made clearer in another presentation, where long-term processes of erosion and landscape denudation in north-west Greece are seen as cumulative palimpsests, but visible through large-scale comparison, despite the coarse resolution of the data (G. Bailey 2007, 210–12). In contrast, the floors of Neolithic houses are seen as true palimpsests, since they are regularly swept clean, with perhaps only small items remaining trodden in; only the final surface would survive for investigation, and might give a misleading impression of the use of the house as a whole (G. Bailey 2007, 203–4). So in following a 'microscopic tendency' (G. Bailey 2007, 209), we are guilty of trying to be inappropriately ethnographic with the Neolithic, and instead we should 'abandon most of the conventional foundations of existing archaeological thinking' (G. Bailey 2008, 28), in order to 'clarify what sorts of theories and questions work best with the structural properties of different sorts of material records at different scales of observation' (G. Bailey 2008, 29).

The key claim is that the choice of timescale affects what we can see. That seems self-evident. Others have also stressed the significance of palimpsests (Lucas 2012, 112–23; 2015). But what is frustrating is

the very simple contrast drawn between the worked examples and the imbalance in their treatment, and the very stark opposition between macroscopic and microscopic scales. What is claimed for Palaeolithic landscapes in Greece seems plausible enough, given the nature of the data. What is claimed for Palaeolithic cave deposits, that they are largely due to ‘mixing of materials’, with blurring of the original pattern’ for the most part (G. Bailey 2007, 204), may also be reasonable, though conjoins and articulations (G. Bailey 2007, 204) could also encourage a more optimistic view. But those carefully considered examples are then contrasted with a single, idealised scenario for Neolithic house floors, lacking sequence, context or any kind of possible variation. Nor are any of the timescales, far less the methods to construct them, considered in any detail. Long-term erosion is mooted simply over 100,000 years (G. Bailey 2007, 211), while the possibility of pinning down ‘moments in time’ other than in exceptional circumstances is rejected (G. Bailey 2007, 209), but nothing is said about the scales in between. We are not told how long the cave occupation lasted; if there were articulations in some of the deposits, was occupation much shorter than often supposed in these contexts? What would a formal age-depth model (see [Chapter 3](#)) tell us? And no timescale at all is given for the abstract and isolated Neolithic house.

I do not want simply to set aside ‘time perspectivism’, unlike many critics (cf. Harding 2005). My argument in this book can itself be seen in a sense as perspectivist, but of a much more optimistic kind. The next step is to set out a much wider range of timescales, with the underlying claim that we have much more variety at our disposal than is often acknowledged. I will then go on to consider how the disciplines of archaeology, history and anthropology deal with the available timescales, and how that has affected the choice of narratives employed to make sense of it all.⁴

The abyss of time: the long term

Archaeologists normally see themselves as the guardians of the long term (Robb 2007, 3). That privileges them to ask a series of Big Questions, in a way that no other discipline can, such as about the nature of hominin evolution, settling down and domestication, and the emergence of cities, civilisations and institutionalised power (Binford 1983; Renfrew 2003). Answers to these questions may vary with ongoing research and discoveries, but constitute a fundamental human story over a timescale simply not available to history and anthropology as currently defined. The time depth of this narrative has been validated through the development of both geology and

archaeology. It is hard for us now to recapture the sense of revolution when short chronologies of the world endorsed both by established religious belief and scholarly traditions were challenged by observations of the antiquity of geological formations. One of the friends who accompanied James Hutton by boat to Siccar Point on the south-east coast of Scotland in 1788, where the inversion of layers was clearly visible, later wrote, 'Revolutions still more remote appeared in the distance of this extraordinary perspective. The mind seemed to grow giddy by looking so far into the abyss of time' (Playfair 1805). It is surprising to us now that Hutton's discoveries and their implications were not immediately accepted, but even public presentations to the prestigious Royal Society of Edinburgh in 1785 seem to have fallen on deaf ears (Repcheck 2003, 154–5). But then followed Charles Lyell and others, and by the time of the publication in 1859 of *The Origin of Species*, Charles Darwin refers many times in his chapter 'On the imperfection of the geological record' to the 'long lapse of years' and 'the vast lapse of time'. 'What an infinite number of generations, which the mind cannot grasp, must have succeeded each other in the long roll of years', he writes in Chapter IX. But there is not just fine prose; Darwin calculates a duration for the denudation of the Weald alone of '306,662,400 years; or say three hundred million years'. And he expected his readers to take notice: 'He who can read Sir Charles Lyell's grand work on the *Principles of Geology*, which the future historian will recognise as having produced a revolution in natural science, yet does not admit how incomprehensibly vast have been the past periods of time, may at once close this volume'.

The concept of Deep Time goes back to at least Stephen Jay Gould (1987). We now know that hominin evolution has a timescale of millions of years. Here is the long term, and I believe that this has an unconscious effect on how we perceive time in other periods, even though the Neolithic period in many regions of Europe lasts some three thousand years as opposed to the three million or more of hominin evolution. This may be one of the reasons why Braudel's concept of *la longue durée*, closely allied to a sense of geological time, has been so attractive to many archaeologists. But it is important to distinguish between the overall span of Deep Time and its more detailed content; duration does not need to equate with lack of change. Importantly, though their narratives remain chronologically imprecise, with timescales (constructed by a range of methods, not just by radiocarbon dating, whose current routine limits are up to about 60,000 years: Taylor and Bar-Yosef 2014, 24) extending for the most part for thousands of years at best, if not tens of thousands of

years, Palaeolithic specialists have been concerned in recent years to find a sharper sense of change and difference in the deep past, and to project a greater sense of ‘the deep history of humankind’ (Shryock and Smail 2011, ix). This ‘long history’, to be written by ‘paleohistorians’ (Shryock and Smail 2011, 13), is argued to be accessible through the choice of appropriate concepts such as kinshipping, webs, connectedness and scalar integration; fractal patterns are also argued to be helpful (Shryock and Smail 2011, 19). Another account of the Palaeolithic restricts the start of history to about 50,000 BP, with a following period of 30,000 years when nothing much of significance happened, succeeded by rapid acceleration from about 20,000 BP (Mithen 2003); another model is much more gradualist, centred among other aspects on the shift from instruments to containers (Gamble 2007). This also leads to another call for the abandonment of ‘pre-history’, though the argument here is not based on chronological refinement but the observation that what is important in many kinds of history is modernity, thus requiring a ‘pre’ before it (Gamble 2014, 152). Placing social life at the heart of the historical process, for example through claims for the emergence of universal kinship, is another important step in changing the nature of enquiry (Gamble 2014, 159).

‘Beyond experience of a single lifetime’

The kinds of chronological frame familiar from many Palaeolithic studies would easily subsume the entire Neolithic period in Europe. From a Palaeolithic perspective, it has been suggested that ‘the past 5000 years constitute the realm of “shallow history,” where patterns and events pile up with incredible speed. Keeping abreast of them requires an almost journalistic pace of reportage’ (Shryock *et al.* 2011, 48). That is not always how it can seem within Neolithic studies. There are imbalances in how the character and tempo of change are perceived. Perhaps too much attention has been given to beginnings. As John Robb has put it (2014, 28):

In the social evolutionary tradition which remains implicit in much of the discipline, the big story lines are technology/economy and inequality. This leaves much of the Neolithic as a null category: early agricultural tribes are a baseline about which there is very little to say other than that they farmed and were egalitarian. Once you get past the Origins of Agriculture, you can safely fast-forward a couple of millennia until you get to Metals and Hierarchy.

That may be unfair on the many European Neolithic and Copper Age specialists, and perhaps better reflects perspectives from within British archaeology. One can quibble too about how soon copper was introduced. But the point is well made.

In part, such lumping may come from the nature of the chronological units which have been the standard currency of Neolithic and Copper Age studies now for a very long time. I refer of course to the culture concept. ‘Culture history’ (a term whose connotations I will be seeking to alter through this book) long preceded radiocarbon dating, but has happily survived alongside it: I believe that our accumulated, detailed knowledge of material culture patterning is of vital importance. But the combination of the two regularly produces cultures with a duration of several centuries. Even phases within that, revealed by typology and other means, are often at least a century in duration; according to Frank Siegmund’s analysis (2012, Abb. 1), though he does not go into the detail of how duration has been estimated, the median Neolithic phase was some 100 years. I will look at that detail in the next chapter, and at the whole chronological edifice within which the European Neolithic is currently studied. There are very few, if any, situations where the culture concept has been done away with entirely, and very few, if any, where the normal chronological framework is more precise than a century or a couple of centuries (the LBK being one obvious exception, which we will come to in due course, especially in [Chapters 4 and 5](#)). Even in the Alpine foreland, where dendrochronology provides remarkably detailed site biographies accurate to the calendar year (and more often than not, of very short duration, as I also explore in the next chapter), very similar chronological-cultural charts for each region are normal (Ebersbach *et al.* 2017, fig. 2; see also Stöckli 2016, Abb. 81–2).

This is not all the result of the ways in which chronology is constructed. In *The early Mediterranean village*, John Robb (2007, 3) has urged that we look beyond the temptation to create a kind of ethnographic present, and look at ‘how humans make their history on a scale beyond experience of a single lifetime’. Again perhaps from a perspective in British archaeology, explanation of long-term change is seen as a major gap in recent archaeological theory (Robb 2007, 287). For the Italian Neolithic, this results in a model ‘for observing the historical workings of practice’ at ‘neither the span of decades nor of millennia, but on the order of a few centuries’ (Robb 2007, 294). That leads to a narrative in which the chronological currency is still big units, such as the Early, Middle, Late and Final Neolithic, and the Copper Age, with generalised periods such as ‘earlier sixth millennium to later fifth millennium’, ‘later fifth to earlier fourth’, and so on (Robb 2007, 337). There is little here on the tempo of change, and more emphasis on stability (Robb 2007, 338), though there is

important discussion of historical trajectory within an envelope of possibilities (Robb 2007, fig. 58). There are other distinguished surveys of Mediterranean prehistory which generally operate at similar scales (Guilaine 2003; Broodbank 2013), though at one point Cyprian Broodbank (2013, fig. 5.21) discusses the tempo of the spread of the Neolithic through the Mediterranean at century intervals, and Jean Guilaine (2003, 103–12) is also well aware of the possibilities of rapid change and varying tempo.

It is not as though shorter timescales are ignored. In another account, Robb and Tim Pauketat (2013, [fig. 1.1](#)) set out what is probably the most detailed and explicit multi-scalar perspective so far ([Fig. 2.3](#)). A series of scales are envisaged, from the broad and long setting of particular cultural worlds, to recurrent ways of doing things (traditions, institutions and practices), to historical landscapes ('local bundles of mutually reinforcing institutions and practices'), to local political histories and cycles, and finally tipping points and rapid general transitions. Here then are much more differentiation and much more potential tempo; local histories and cycles are seen to go at quite a pace, and tipping points can be 'moments of rapid transformation' (Robb and Pauketat 2013, 25). Cultural worlds can last for over a millennium, and the duration of historical landscapes is seen as a few centuries. Importantly, this multi-scalar view advocates 'the idea of history as multiple genealogies of practices and the vision of history as a palimpsest of qualitatively different processes' (Robb and Pauketat 2013, 26). Though the language is not dissimilar to that of Geoff Bailey, as discussed above, the differences are striking. In the final discussion in this paper, however, the authors come down on the side of 'big histories'; 'small histories' are seen to rely implicitly on larger narratives (Robb and Pauketat 2013, 31). And there is a final argument (which I will come back to in [Chapter 7](#)), based on the modern conditions in which research is funded from the public purse, that we 'cannot afford to restrict our field of study to small scale out of a reluctance to commit ourselves politically' (Robb and Pauketat 2013, 33). That seems to validate the scale of a few centuries above others. This also appears in an important long-term and explicitly multi-scalar study of changing beliefs about the human body, where in the end synthesis and the big picture ('the road less taken' by recent historians: Harris and Robb 2013, 25) seem to be preferred to particularism, even though the aim is 'not to privilege a single scale of analysis, but to look at relations between them' (Harris and Robb 2013, 27).

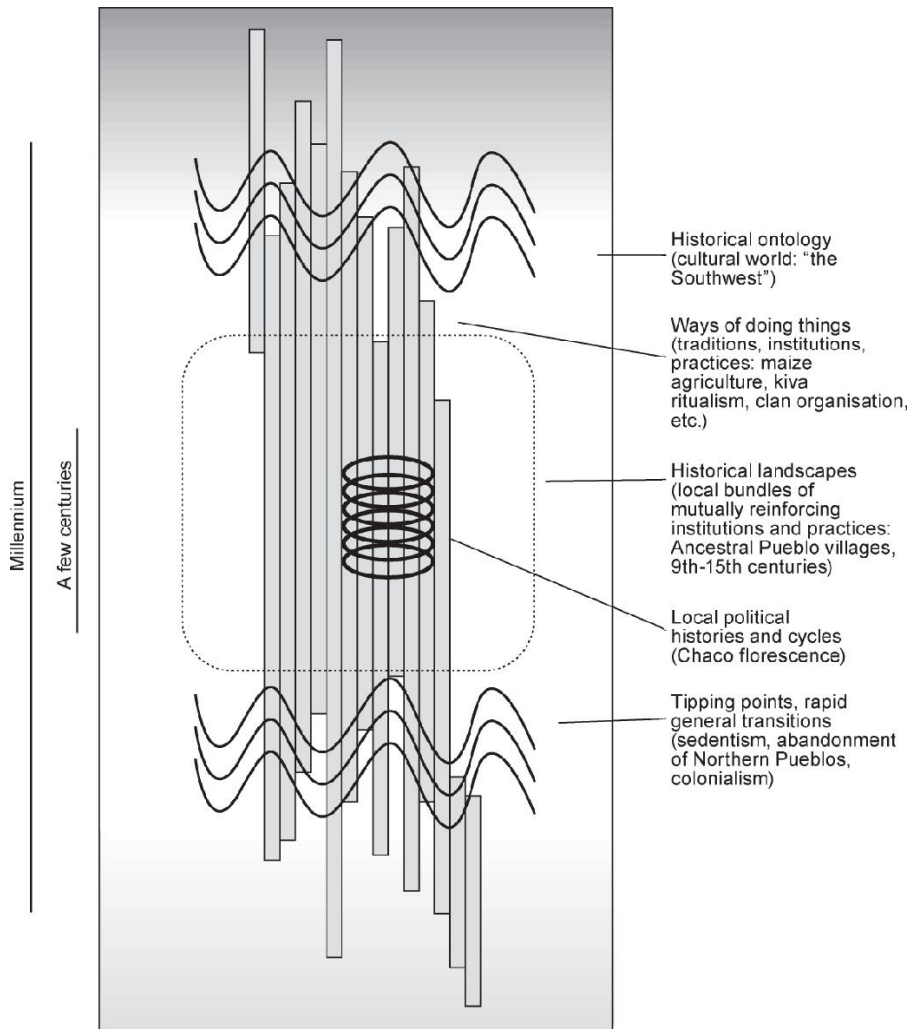


Fig. 2.3: Some of the temporal scales of history. After Robb and Pauketat (2013), by kind permission of John Robb.

Lifetimes and generations

For these different reasons, it is perhaps therefore not surprising that many analyses of the Neolithic have produced narratives of change which unfold at a comfortable scale and pace. In this approach, precision seems not really to matter. General alignment of parallel cultures or cultural units, and the painting of the big picture, are more of a concern. But given that history, however we narrate it, is in the end formed by the actions or agency of people, we should also try to think at more human timescales.

In his *Leviathan*, written in the mid-17th century in the aftermath of the English Civil War, Thomas Hobbes famously warned that without civil society life would be 'poor, nasty, brutish, solitary and short'. I

will discuss questions of inter-personal and inter-group conflict and violence at various points in [Chapters 4–6](#); the record is variable (Schulting and Fibiger 2012), with no clear signs in the Neolithic for the universal mayhem feared by Hobbes, but the evidence does suggest that lifetimes were short. The biblical ‘three score years and ten’ would have represented a long lifetime in Neolithic terms. The life tables constructed on the basis of the skeletal record suggest in fact that the average lifetime may well have been significantly shorter, around 40 years or fewer (Robb 2007, 40–3; cf. Weiss 1973); problems in the reliable measurement of ageing from ancient skeletal evidence should certainly be noted (Mays 1998, 50; Falys and Lewis 2011). There were probably differences between the sexes, with significant female mortality in the child-bearing years, and there was high infant mortality. In previous studies, we have taken 70 years to represent a long lifetime in the Neolithic (Bayliss and Whittle 2007; Whittle *et al.* 2011), and that figure can stand as a useful measure, though it is likely to have been at the upper end of survival. If many adults died in their 30s, 40s or 50s, it gives added emphasis to the significance of individual agency and the transmission of ideas and practices. If we want to catch something of the tempo of Neolithic lives and the nature of lived experience, we need to be able to construct chronologies with a matching precision.

In previous studies, we have also used the notion of the generation as a useful human unit of time, and as a working figure proposed 25 years for the span of a generation (Bayliss and Whittle 2007; Whittle *et al.* 2011; see also Heyd 2007, 334). That figure is useful, for thinking about agency, continuity, tradition and change at much finer scales within centuries, but it should not be regarded as absolute. We based our estimate on two studies, of Icelandic genealogies and Ashkenazi Jewish population (Helgason *et al.* 2003; Slatkin 2004); others could have been cited (e.g. Weiss 1973; Tremblay and Vézina 2000). We noted variation, with both biological and social factors to be taken into account, including the age of the onset of female puberty and the span of female fertility on the one hand, and social notions of adulthood, reproduction and succession (Whittle *et al.* 2007, 131). Wider comparative studies, of increasing importance for their implications for the rate of genetic divergence (Fenner 2005; see also Moorjani *et al.* 2016) suggest that the 25-year figure may not be far off the mark as an average, but needs to be reviewed critically. The substantial study by Fenner used data from modern nation states and recent hunter-gatherer societies; it found (as had Helgason *et al.* 2003) that the female generation interval was shorter than the male, and

that both intervals have often been underestimated (Fenner 2005, 415–16). The conclusion reached was that ‘values of 25, 28, and 31 years should be used for the female, overall, and male generation intervals’ (Fenner 2005, 421).

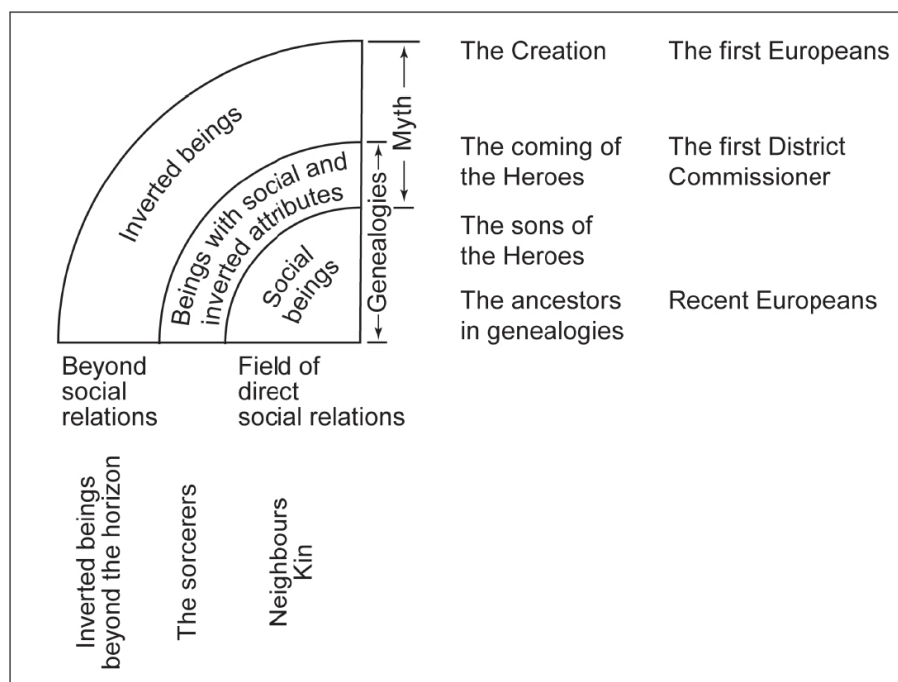


Fig. 2.4: The Lugbara worldview. After Middleton (1960).

Both lifetimes and generations can be social or cultural constructs as well as biological measures. Ancient Egyptians had an idealised concept of the lifetime (*ch^cw*), which spanned 100 years plus one or two decades for the acquisition of ultimate wisdom (Meskell and Joyce 2003, 5). Ethnographic studies show many instances of people reckoning social time in generations, but not necessarily with strict measurement of time. The Tiv of Nigeria reckon backwards up to 17 generations, and those who could do so acquired considerable reputation (Bohannon 1952), while the Lugbara of central Africa (across the Congo–Uganda border) had a cosmology going back through forebears, tracked through genealogies, but on to hero figures and the creator figure (Middleton 1960) (Fig. 2.4). Study of the Borano Oromo has shown how crushing control of marriage by senior males postponed the chances of male reproduction until as late as 40 years of age (Megerssa and Kassam 2005). So we should not be complacent about the working figures currently available, and there is

presumably very fruitful research to be done, for example in Neolithic cemeteries, combining high-resolution ageing techniques and high-resolution formal chronological modelling. There can also be something of a disjuncture between this appealing notion of the human generation, rather arbitrarily applied to modelled date estimates, and the overall interpretation of change. Did the historical process alter to a generational pulse, other than in the succession of kings and leaders? The generation interval applies to the small social reproductive unit of family or immediate kin, but births and deaths occur all the time. In his study of the Lugbara, John Middleton (1982, 134) noted the frequency and familiarity of deaths in areas of dense settlement, though the converse seems to have been the case in areas of low-density and dispersed settlement, where deaths attracted much less social attention. On the other hand, when we come to look in [Chapter 4](#) at questions of the succession and duration of houses, monuments and material styles, and in [Chapter 5](#) at the attempted creation and maintenance of social difference in a variety of situations, generational intervals on the kind of scales discussed above will be seen to be central to understanding more of the nature and tempo of change.

The reach of personal and social memory

Thinking about and with lifetimes and generations should thus help us to unpick the scale of what Braudel called *conjonctures* or social history in considerably more detail. But it is important not to see these notional scales so far discussed as wholly independent, even though the underlying argument is that things can look different at different scales. Linking varying times is the reach of personal and individual memory, and archaeology has usefully caught up in recent years with studies of both remembering and forgetting (Bradley 2002; Jones 2007; Borić 2010). Straightforward notions of personal memory enable inferences of transmission at lifetime and inter-generational scales. Someone alive as a youngster in southern Britain in the late 38th century cal BC, for example, at the time when the first enclosures were dug, could have been told by a grandparent of the circumstances of early long barrow constructions in the first part of that century, and in turn could have passed on to grandchildren in the middle part of the 37th century direct memories of the events connected to the initiation of enclosures. The reach of personal memory could therefore be reckoned as comfortably over a century. Memory is fallible, however, and we know from studies of the transmission of ritual knowledge, for example in Fredrik Barth's classic account (1987) of the Ok people of highland New Guinea how the periodic requirement

to pass on secrets to the next cohort of initiates, after a lapse of a decade or more, could cause much stress for the seniors responsible, and how their imperfect performances were one significant source of variation over time.

In turn, there need be no absolute distinction between personal and social memory (nor between other many proposed classifications of memory: Whittle 2010), since things can be deliberately forgotten (Forty and Küchler 1999), and what is chosen to be remembered is often socially conditioned. Richard Bradley (2002, 8) proposed 200 years for the active transmission of memory in oral (as opposed to written) traditions, and that could indicate the span of social or cultural memory discussed from the studies of Halbwachs onwards, and overlap with the social history of Braudel, for example. There are potential counter examples, indicating the possibility of much longer spans, for example in the claims that Aboriginal stories reach back thousands of years to the early Holocene flooding of the coast of Australia (Nunn and Reid 2015; but see also Hiscock and Faulkner 2006), that European folk tales could retain features of a Bronze Age world (da Silva and Tehrani 2016), or that more than 100 generations of Tamil poet-preachers have passed on sacred verse (Dalrymple 2015). Perhaps such transmissions are not impossible, but whether the retelling of myth and story evokes any sense of precise timescales among tellers and listeners is much harder to say.



Fig. 2.5: Slow time in the modern Romanian villages of Măgura and Schitu. Photos by Douglass Bailey.

This puts our narrative powers on the spot. A distinction between invented myth and remembered history has been evoked in discussion of the sequence of development at Çatalhöyük (Hodder and Cessford 2004; Hodder and Pels 2010; cf. Bayliss *et al.* 2014; 2015; and see further below). Another example, already mentioned in [Chapter 1](#), is the uncanny resemblance of the second architectural phase of the Wayland's Smithy long barrow to the form of the West Kennet long barrow, probably built two centuries earlier. Was there active memory of the use of the earlier construction, or could people in the later situation simply evoke a sense of something older, which had passed out of active memory and had no specific timescales attached, simply by looking at what remained? A final example here has much concerned British Neolithic specialists. Did the form of early long barrows and cairns in Britain evoke that of continental Danubian

longhouses? The formal similarities have often been pointed out (Hodder 1984), but the probable chronological gap between the last longhouses and the first long mounds or cairns which might have evoked their memory has widened now to several centuries (Whittle *et al.* 2011, 872). Are the resemblances more apparent than real? Could the form of long mounds and cairns relate to much more immediate predecessors, such as long mounds in northern France, or the houses now known in larger numbers within Britain and Ireland themselves? Or does this possible linkage betray the existence of long-lasting myth and story, powerful enough to affect how people thought and acted in much later circumstances of change?

Even finer scales: the flow of daily life, moments and traces, and events

Memory and forgetting involve looking back; archaeologists have been comfortable with trying to tracing these, even though we have not, as discussed above, been altogether successful with longer chains of remembering and their effects on action. We have been much less ambitious with more immediate timescales, but there is plenty to think about.

In a paper called ‘Eleven minutes and forty seconds in the Neolithic’, Douglass Bailey and Melanie Simpkin (2015) have challenged whether we work at the appropriate timescale. Despite improved excavation techniques and refined dating (this is not specified in detail) of an early Neolithic context at Măgura in southern Romania, ‘no increase in our efforts would get us any closer to the time of the prehistoric people we sought’ (D. Bailey and Simpkin 2015, 190). Their hypothesis is that life and change occur in ‘real time’, by implication a very slow time, which is reduced or compressed through ‘chronotypic fabrication’, fudging reality; though we ‘talk grandly about agency, intention, belief, mentality and the individual’, we miss out the scale of lived action (D. Bailey and Simpkin 2015, 190). Their response was to try to imagine the flow of existence, ‘the slow, almost static pace of daily life’, in this instance by filming what happens on the outskirts of a modern Romanian village, and dissecting, using notation usually employed in dance choreography, the rhythms and directions of people on the move about their daily business (Fig. 2.5).

This seems both pessimistic, seeming to share some of the time perspectivist views set out above, and optimistic, in adopting the power of imagination and the stimulus of an alternative medium. It is revealing, however, that the approach is not followed further with more archaeological evidence. I argue that this can certainly be

thought about much further. This will not necessarily be possible everywhere and in all settings. But consider what a rich array of short-scale temporalities are potentially available in settlements with defined buildings and spaces in between. The time perspectivist example of the single abstract house and the understandable concern with palimpsests together miss the point. Within a generational timescale, as suggested by both formal chronological modelling of radiocarbon dates and the seriation of decorated pottery, patterns of different activities can be detected among and within houses. At fifth-millennium Okolište in Bosnia, the residues in adjacent burnt houses on the tell suggest varying emphases on different subsistence activities (Müller *et al.* 2013), while in the LBK settlement of Vaihingen in south-west Germany, it appears that groups of houses, possibly representing clans, were distinguished by subtle differences in the location and intensity of use of their cultivation plots (Bogaard *et al.* 2011). There is now a formidable battery of scientific techniques with which to examine diet and the movement of people and animals (many reviewed in Whittle and Bickle 2014); high-resolution isotopic analysis of teeth, for example, can give a clear sense of seasonal variation, across the short timescales of the lives of cattle and sheep (Knipper 2011; Balasse *et al.* 2014; and see [Chapter 7](#)). In some situations, a very wide array of evidence for seasonality is available (e.g. Whittle 2007). So at the least a strongly contextualised picture of the comings and goings of daily life can be built for some situations. It will not normally be possible to say which eleven minutes were involved, and the scenarios created will probably normally be cumulative than strictly synchronic, but in more cases than we have so far admitted, we should be able to confine them to much more particular times.

That is not universally true. In the Alpine foreland, there are the twin advantages of high-quality preservation of organic remains and high-resolution dendrochronology. The burnt settlement of Hornstaad-Hörnle IA (see [Fig. 3.2](#)) gives a snapshot of village life at the calendar date, dendrochronologically measured, of 3910 BC, having been founded in 3918 BC (Billamboz 2006; Styring *et al.* 2016). The Hornstaad fire seems to have taken place in late summer or early autumn, given the abundant deposits of processed grain, which once again vary from house to house, and house group to house group (Styring *et al.* 2016, fig. 6). Even here, we cannot claim that the whole archaeological deposit is synchronic; these and many other Alpine foreland settlements (as discussed further in the next chapter) lasted for up to 15 years or so.

Doug Bailey and Melanie Simpkin stressed the slow pace of Romanian village life; this certainly comes over strongly in the accompanying film, as well as in the stills given in their paper. We could object that this reflects a particular historical phase, in particular circumstances of political, social and economic change in modern Romania. Was life in villages always so slow? Perhaps more of a challenge in fact than the timing and absolute duration of settlement life are its temporalities. Numerous comparative and theoretical studies suggest there are not only seasonal rhythms and varying moods to daily life (Bourdieu 1977, fig. 9; M. Harris 2000) but styles of living. Patterns of daily life can be seen, for example, as forms of resistance (de Certeau 1984), carried out within a particular aesthetic (Bourdieu 1984; Gregg and Seigworth 2010). In her anthropological studies, Joanna Overing (2003; Overing and Passes 2000) has stressed the importance of defining the values within which daily life is performed; in many of the Amazonian contexts which she has investigated, there is an emphasis on quietness and restrained behaviour, in part as a counter to notions of a potentially very hostile and dangerous universe outside the settlement.



Fig. 2.6: Moments in time: the first Mesolithic footprints to be discovered, by the late Derek Upton, on the tidal foreshore of the Severn estuary. Photo by the late Stephen Aldhouse-Green, and by kind permission of Cadw. A younger (early 1980s) version of the author provides a scale.

One answer to how we might think about this further will rest again on a sense of context. We could enhance that if we were better at reading traces. I noted above the pessimism in the perspectivist view about capturing particular moments in time. I think that is misplaced. Though the challenge of palimpsests is ever present, I believe that much more of the archaeological record is formed from particular actions and deposits than we allow. If we consider the digging of very regularly aligned postholes for the setting up of a big longhouse, or the laying of a corpse and surrounding offerings and mementoes in a grave, or the burning of a house, or the placing of articulated animal remains in the base of an enclosure ditch, or human footprints across inter-tidal deposits (Aldhouse-Green *et al.* 1993) (Fig. 2.6), or an

arrow piercing the third lumbar vertebra in a 37th-century cal BC killing (Benson and Whittle 2007) (Fig. 2.7), a strong case can be made for archaeology as an almost inexhaustible archive of the short-term and the particular, rather than of the long-term and the general as so often advocated. But not only timing but reading these innumerable moments is difficult. Sometimes in our own world we are also baffled, like the frustrated neighbour and narrator in search of the elusive Holly Golightly in Truman Capote's *Breakfast at Tiffany's*.⁶ Sometimes we can read the clues better, as in the motel scene in *Travels with Charley in search of America*, where John Steinbeck pieces together an illicit relationship from the small traces and signs given by a pillow, a whisky glass and a human hair. But how many people were around the grave at a particular Neolithic funeral, and was the man from Ascott-under-Wychwood shot in an open fight or the victim of an ambush? There is also the philosophical problem of how to define the 'present' (R. Jenkins 2002; Hoy 2009).



Fig. 2.7: Moments in time: leaf arrowhead in the third lumbar vertebra of Individual B2, Ascott-under-Wychwood long barrow, Oxfordshire. After Benson and Whittle (2007).

Can we also get at events in Neolithic and Copper Age archaeology? The pessimistic position would once more be that they are nearly always out of reach. The optimistic view is again that there are a lot more potential events in the archaeological record than we have allowed. The further challenge is that the definition of events rests on

a sense not only of particularity, but also of significance. One view of events is as '(1) a ramified sequence of occurrences that (2) is recognized as notable by contemporaries, and that (3) results in a durable transformation of structures' (Sewell 2005, 228; Bolender 2010), but this seems to confine them to being tipping points, rather than regarding them as both more and less significant moments in longer chains of complicated process (Gaddis 2002, 65–6). Candidates could include both beginnings and endings, for example the layout and construction of an enclosure, surely eventlike if it involved substantial numbers of people, or the burning of a village, involving not just one house or household, but a community and potentially neighbours, allies and enemies. Some diggings and some burnings may have been more significant than others; the first enclosure to be constructed in southern England might have been more resonant than successors, and if the Vinča-Belo Brdo tell really was the last of its kind to be abandoned, in troubled circumstances, it too could have been seen as particularly significant. Rapid change in material culture with the introduction of Bell Beakers has also been considered as event-like (van der Waals 1984), and I look at other examples of this kind in [Chapters 4](#) and [6](#). Even if we can only get as close as a sense of eventful horizons (Beck *et al.* 2007; Whittle *et al.* 2010), that in itself radically changes the kinds of history which we can create in our narratives.

The times of archaeology, history and anthropology

So far, I have argued that looking at things at varying scales makes a difference, and that we have more scales available to us in archaeology than we often allow. Despite the pessimism of some formulations of the time perspectivist approach, there is an increasing willingness among many researchers, including those working on the Palaeolithic, to think of history as an appropriate frame of reference. But can we really be like history, and what kind of history can we write for the Neolithic period in Europe?

History in archaeology

It is not in fact new to link archaeology and history. An earlier generation of archaeologists such as Childe, Clark and Piggott probably saw themselves as working within an ultimately historical framework, not least in the days before the acceptance and expansion of radiocarbon dating, when short chronologies prevailed. *What happened in history* remains one of Childe's (1942) most striking titles, and the book begins with a chapter boldly called 'Archaeology and History'. Disappointingly, however, apart from a spirited attack in the

very first sentence on the incompleteness of written history, there is virtually no theory here; instead the chapter serves as a general outline of human development within a very broad chronology for prehistory.

In more recent times, the theme of the link between history and archaeology has come back from time to time, without ever being established or widely accepted enough to become mainstream. In a now fascinating paper of 1970, at a time when debate was intense about new or processual archaeology, Bruce Trigger made a strong case for prehistoric archaeology as a historical discipline, not just descriptive but fully open to interpretation like the best of then contemporary historians such as Carr and Elton; this 'idiographic' approach was counter to the 'nomothetic' generalising of social science and processual archaeology in the shape of Lewis Binford and David Clarke (Trigger 1970, 30–1). Presciently, he noted that a growing number of historians were 'willing to regard themselves as scientists' (Trigger 1970, 31), a point I will come back to below. But while explicitly claiming archaeology as history, he also warned strongly against 'unwarranted speculations in a desperate attempt to produce something resembling narrative history' (Trigger 1970, 31). Typically enough for an article of this kind, there was no sustained case study. Trigger's own detailed work on the Huron and the Iroquois Confederacy, *The children of Aataentsic* (1976), gives a good idea of the kind of richly documented history he went on to write, but the narratives there rely much more on historical sources than archaeological evidence.⁷ Nor was the 1970 paper much commented on, either in the pages of *Antiquity*,⁸ or in the subsequent big conferences of the day (e.g. Renfrew 1973a). Other papers followed (Trigger 1982; 1984; 1989) but never became part of mainstream views, though they were noted by Pauketat (2001, 73), as discussed further below. Perhaps this was in part because of their more general tone. One cites Childe and Marx in the context of discussion of historical knowledge as an understanding of 'how and why specific societies developed as they did' (Trigger 1984, 289), but does not go further into the detail of historical writing, as in the earlier paper.

In one of the many twists and turns of the subsequent post-processual archaeology, Ian Hodder (1987a; 1987b) was to claim archaeology as long-term history. Lamenting that archaeology had become 'to a great extent ahistorical' (Hodder 1987a, vii), Hodder (1987b) sought to incorporate a much more explicit sense of sequence and the long term in the understanding of material culture. He noted the position of Trigger, though he did not quote the 1970 paper, and

harked back to Childe. He discusses Braudel at some length, but tends to contrast the very long term simply with 'the myriad of individual events excavated by archaeologists' (Hodder 1987b, 6); a very general programme was proposed, in which 'Braudel's scheme ... is best replaced by an examination of the relationships between structure (of various types and scales) and event (of various types and scales)' (Hodder 1987b, 7). There is brief mention of 'history from the inside', as advocated by Collingwood, 'to grasp the intentions, values and organisational schemes within human action' (Hodder 1987b, 2). The other papers in *Archaeology as long-term history* do not fully cover the range of scales I have already discussed, choosing either the long term or the short term.

By the time of *The domestication of Europe*, Hodder (1990) had reverted to a much more generalising narrative about the playing out of structuring principles (*domus*, *agrios* and *foris*) through the Neolithic sequence in Europe. An interest in Collingwood was to resurface briefly in *Reading the past* (Hodder 1986), though a method to get inside the minds of past historical actors was never set out. Different approaches to change and history have been tried in discussion of the results of sustained research at Çatalhöyük. One trope has been that of very slow change. In one account, Hodder envisaged long, slow changes to the big picture, taking place through 'myriad small steps' and 'infinitesimal moves in daily life and daily practices' (Hodder 2006, 251, 236). In a parallel way, Andrew Sherratt, while perfectly well aware of course of the possibilities within Neolithic studies for fine chronological resolution, not least as provided by the dendrochronologies of the Alpine foreland, explicitly called for a return to grand narratives, to seek connections and consequences at larger scales, both temporal and spatial, beyond what he called the myopic focus of post-processualist approaches (Sherratt 1995; 2004). His phrasing is instructive: for example, 'large structures', 'systematic long-term change', 'coherent, long-term change', and 'directional sequence' (Sherratt 1995, 3).

Another Çatalhöyük theme has been the idea of history as the active maintenance and social use of memory, in the form of the houses which make up the tell. From the notion of the house in particular as the site for social memory (Hodder and Cessford 2004, 31; cf. D. Bailey 1990), there developed the idea of history houses, particular structures in which more transcendent knowledge and symbolic capital were accumulated than in others, and in which histories were amassed (Hodder and Pels 2010, 164). It is revealing that although the question of the nature of what such histories might constitute is

raised (Hodder and Pels 2010, 179), only rather general answers are given. The main strand is that history is to do with direct memory of particular events, in contrast to ‘habituated and/or mythical memory’ (Hodder and Pels 2010, 182–3); this is seen to run over several generations (Hodder and Pels 2010, 183), and eventually, in upper levels, this kind of memorialisation became less frequent as houses became gradually more individualised. So the idea of history is closely linked to memory, within relatively short but in the end here imprecisely defined timescales. It may also be revealing that critique of ‘history houses’ at Çatalhöyük focuses on the detail of the buildings and their contents, not on the narrow definition of history (Carleton *et al.* 2013; Hodder 2016).

Advocacy of the *Annaliste* approach to history has been another strand in archaeology. John Bintliff (1991; see also Knapp 1992) has described and discussed in detail its emergence and development, which goes far beyond the work of Braudel himself. Important though that was for setting out the threefold schema already noted above of *la longue durée*, medium-term structural history and the history of events (Bintliff 1991, 6–9), other *Annalistes* and other historians wanted to examine the relationship between them much more critically. For other *Annalistes* both structural history and short-term processes, recounted in closely written, contextualised accounts such as in the classic *Montaillou* of Le Roy Ladurie (1978), were much more important than the very long term. Even though he claims ‘a more limited situation in prehistoric archaeology’, Bintliff (1991, 18–19) suggested optimistically that ‘using problem history we can look especially at structural change as the locus for tracing the bow-waves of events’, that is, the ‘problem of the interplay of different timescales in the past’ (Bintliff 1991, 14). The accompanying case study on the landscape of Boeotia, however, is disappointing from my perspective in this book, since it simply contrasts historical sources such as Polybius with very brief reference to the outcome of detailed surveys, which suggest ‘cycles of expansion and contraction operating ... at wavelengths of 400–500 years over a far longer time-scale’ (Bintliff 1991, 26).

The same approach underpins a later account of the archaeology of Greece (Bintliff 2012), and another single case study, of the ancient and modern city of Argos in the Peloponnese, has sought to combine long-term, medium-term and short-term approaches, including notions of the event (Bintliff 2010). The timescale is again considerable: over 7000 years, with some events effecting major transformations, but others not; medium-term cycles of urban rise and decline are

suggested to have been ‘in deeper harmony with longer-term structural oscillations’ (Bintliff 2010, 130). Medium-term processes, however, are defined as ‘phases of several centuries’ (Bintliff 2010, 121). For all the talk of events, considerable imprecision remains. Concluding another volume of papers on the value of *Annaliste* approaches, Andrew Sherratt (1992, 137) claimed that human agency had been lost sight of, but for all the detail available for discussion, he appeared in the end to advocate ‘a large scale’ and ‘a wider spatio-temporal framework’ (Sherratt 1992, 139–40).

Elevation of historical explanations has also been promoted in connection with practice theory or ‘historical processualism’ (Pauketat 2001; 2007; Pauketat and Alt 2005). This has been part of a move to turn away from the generalising approaches of processualism. ‘Today, history matters in archaeology’ (Pauketat 2001, 74; and references). This has been closely linked to a theory of practice, practices being seen as generative processes of change, and based on ideas of agency and habitus, founded on Bourdieu and Giddens (Pauketat 2001, 74, 79). Case studies of the wider Mississippian phenomenon recognise varying timescales, from the ‘event’ of the emergence of Cahokia, occurring ‘within a short period of years’ (Pauketat 2001, 83) to the possibility of ‘microhistories’ of cultural practices at household level (Pauketat 2007, 210). Pauketat asserts (2001, 86) that ‘history is not a succession of cultural structures, institutions, or elite dynasties, and historical processes are not the abstract transitions or freak events between structures, institutions and organizations’, but rather ‘the process of cultural construction through practice’ (2001, 87).

The preference in this kind of history is clearly for the locus of generation of change through practice, ‘in microscale actions and presentations’ (Pauketat 2001, 87). How these play out at wider and longer scales is not really explained (cf. Sahllins 2005), nor is the construction of Mississippian timescales critically examined in detail; I think (as an outside observer) that this has largely rested on a combination of artefact typology and the visual inspection of radiocarbon dates. Later accounts by Tim Pauketat have become more complicated, because the agency of people is joined by the agency of things, in notions of bundling (Pauketat 2013). I will come back to this in [Chapter 4](#). Another example of the uptake of notions of history in American archaeology is reflected in the title of a recent monograph on shell middens in Florida: *Constructing histories* (Randall 2015). This is framed in terms of social history, and constructing narratives by ‘detailing genealogies or histories of practices’ and ‘tacking back and forth between many lines of evidence ... at multiple

scales' (Randall 2015, 81). The chronological units deployed, however, are of the order of several centuries. Alice Beck Kehoe also explicitly addresses the question of how to 'construct a history', in her survey of the North American sequence (Kehoe 2017, 225–9).

A spirited critique of time perspectivism also makes use of the notion of 'genealogies of practice' (Harding 2005). These are seen to bridge the divide between long-term structural history (as represented by Braudel) and events and the short-term (as partly represented by Le Roy Ladurie), locating the experience of time in its social context (cf. R. Jenkins 2002, 277). It is asserted that there is 'no conceptual opposition between synchronic and diachronic temporality, because any system, as it unfolds through time, will not only be reproduced, but also constantly distorted or modified, endlessly producing novel terms of understanding for those experiencing each new moment of existence' (Harding 2005, 98). How the measurement of passing time is to be achieved is not explored, however, nor how narratives combining different timescales might be written.

Broodbank's firm characterisation of 'prehistory' as early history is also to be noted (2013, 21; with Braudel, no less, quoted in support of this view). In another context, Braudel memorably argued (1980, 47) that 'the historian can never get away from the question of time in history: it sticks to his thinking like soil to a gardener's spade'. There is a quite healthy strand in archaeology of seeking links with history, but even the selected examples noted above show many gaps, and a tendency to refer to a very general notion of history. What do historians themselves have to say about history?

Historians on history

It is important therefore to look beyond the *Annaliste* approach and to consider briefly what other historians are saying about history. Bruce Trigger (1970) sensibly cited Carr and Elton, leading figures of his day who had a now classic debate about interpretation and notions of progress (see Tosh 1991; K. Jenkins 1995). The big story of what has happened since those days starts with the post-modernist assault on history, and continues with the subsequent recovery of confidence, tempered by lessons learnt from intense debate about narrative, and goes on with an increasing diversity of approaches. This all has implications for how archaeology might be aligned with history. Since this is not a history textbook, my account is deliberately selective (like all narratives).

That a leading historian, Richard Evans, who has written extensively about German history and more recently on the 19th century (R. Evans 2016), should give a book published in 1997 the

title of *In defence of history*, gives some idea of the fierceness of the post-modernist attack on history. The basic notion, inspired by French and other post-modernist ideas on text, was that the past could not be apprehended separately from the texts and other written sources which historians study. (A similar, 'presentist' view continues to be found in some kinds of archaeology: see Olivier (2015).) In one account, the somewhat formidable list of sources driving this includes 'hermeneutics, phenomenology, structuralism, post-structuralism, deconstructionism, new historicism, feminism and post-feminism, post-Marxism, new pragmatism, postmodernism and so on' (K. Jenkins 1995, 3); the upshot is that Carr and Elton were claimed as 'passé' and 'unmistakably modernist ... and generally irrelevant to an understanding of what history is' (K. Jenkins 1995, 3, 6). This was not just because past history was seen as over-dominated by upper-case History, 'a way of looking at the past in terms that assigned to contingent events and situations an objective significance by identifying their place and function within a general schema of historical development usually construed as appropriately "progressive"' (K. Jenkins 1995, 8). 'Lower-case' or smaller-scale history was similarly dismissed, and 'the whole "modernist" History/history ensemble' was seen as a 'self-referential, problematic expression of "interests", an ideological-interpretive discourse without any "real" access to the past as such' (K. Jenkins 1995, 9).

If the past is not written history, in this argument, if the traces of the past as the past are not literally represented, then that historiography becomes reduced to a series of ideas and theories that historians have about turning the past into history (K. Jenkins 1995, 21). This led to close examination of narrative. Hayden White among others argued that the encoding of events, dates and agents into narrative produced patterns of meaning separate from the literal facts (K. Jenkins 1995, 24); narrative was seen fundamentally as a kind of allegory (White 1987, 48), which could be classed as tragic, heroic, farcical or other. A wider schema (White 1973; K. Jenkins 1995, 149–50) saw the progression from traces to chronicle, and the transformation of chronicle into story, through different elements of argument (formal, organic, and so on), different modes of emplotment (romance, comedy, tragedy and satire), varying ideologies (conservative, liberal, and so on) and finally different tropes, including metaphor, metonymy, synecdoche and irony.

This relativism recalls Edmund Leach's gloomy dictum (1982, 53), from a similar time, that anthropologists are 'bad novelists rather than bad scientists' (and I will note anthropology's own post-modernist

travails below). The case for the defence of history goes back to Carr's view (1963, 30) that history is 'a continuous process of interaction between the historian and his facts, an unending dialogue between the present and the past'. Evans (1997, 163) has stressed the diversity of historical approaches,⁹ and pointed to the failings and contradictions of post-modernist sceptics and critics themselves (R. Evans 1997, chapter 8). He has underlined the importance of sources and archives, and concludes that 'it really happened, and we really can, if we are very scrupulous and careful and self-critical, find out how it happened and reach some tenable though less than final conclusions about what it all meant' (R. Evans 1997, 253).

If history has survived as a discipline, older and wiser, it is nonetheless striking that more than a little of the post-modernist critique has been absorbed into how it is now characterised. This is explicitly acknowledged by Evans (1997, 248). John Arnold (2000, 13) has claimed that 'true stories' can be written about the past, in the sense that they must be consistent with the available evidence, but acknowledges that they are interpretations, placing facts within 'a wider context or narrative'. History is seen as making sense of the mess of life, 'finding or creating patterns and meanings and stories from the maelstrom' (Arnold 2000, 13). It is also acknowledged to be political, in that narratives, for example about medieval history, so often have connotations for our view of the modern world (Arnold 2008, 22). John Lewis Gaddis (2002, 62–6, 69–71, 89, 108) has argued that history can better be aligned with certain of what he calls the natural sciences (astronomy, geology, palaeontology, evolutionary biology and medicine are specifically cited) than the social sciences, in that historians work with particular generalisations, embedded within narratives, rather than embed narratives within generalisations, like social scientists. Narrative is equated with simulation rather than modelling (Gaddis 2002, 65), and can embrace rather than seek to reduce complexity.

In this context, there are two important dimensions to the ongoing writing of history, apart from a continuing commitment to the archives (Trouillot 1995, 58; R. Evans 1997, 249; Arnold 2000, 62; 2008, 26; Gaddis 2002, 107, 114). First, the scope and diversity of historical writing are enormous, and its styles extremely varied; the kinds of true story that can be told about the past are seemingly endless. Open any history catalogue from a major publisher, and a vast range presents itself. The accusation that historians write only History (K. Jenkins 1995, 8) itself now seems *passé*. Women's history, subaltern histories as part of a new social history (Kelley and Smith

2012, 97–101) and an interest in global connections are among more recent developments. One recent survey concluded that ‘what is remarkable today is the attempt of historians ... to achieve the ‘history of everything’ from microhistory to macrohistory’ (Kelley and Smith 2012, 101–2). I should stress of course that historians do not write only narratives. To take just one example, *The world turned upside down*, the brilliant analysis by Christopher Hill (1972) of the beliefs of 17th-century radical groups in England, has only one chapter, ‘The world restored’, devoted to a brief narrative of the defeat of alternative worldviews. But the book as a whole is rooted in the particulars of the mid-17th-century context.

That raises, secondly, the vital question of scale. In thinking about the choice of narrative, John Lewis Gaddis (2002, 33–4, 125–6) has compared the writing of history to the process of mapping. Making maps involves selecting a scale and choosing what to include and what to omit, since putting everything in would result in a map as large as the world itself. That selection is normally determined by the practical purposes to which a map is to be put, and here the analogy with map making begins to break down. On the one hand, historians are potentially confronted by many dimensions, rather than a single flat surface, and on the other, their data can be both overwhelmingly numerous and frustratingly patchy. Though there is now a *Journal of Global History*,¹⁰ its contents are largely about the connectedness of the world and world histories as such are comparatively rare; that may change again with greater attention to themes such as population, environment and disease (Bayly 2012, 3). Not least in order to control their sources and to make sense of the often abundant evidence, many historians are most comfortable with a shorter rather than longer timescale for their narratives. One account refers to ‘the long haul’ as ‘from the late fourteenth to the end of the sixteenth century’ (Lake 1998, 282). Keith Thomas (2009, 4) refers to his concept of the early modern period, from 1530–1780, as ‘so long a period’. The concept of ‘the long 18th century’, extended back for example to the Glorious Revolution in 1688 and on to the defeat of Napoleon 1815 or the Great Reform Bill in 1832, is another example (e.g. O’Gorman 1997). What is perceived as long-lasting by these historians would have been regarded by many ‘prehistorians’, at least until recently, as impressively short.

Another manifestation of historians’ concern with scale comes in debate about over-neat periodisation, symptomatic of many former grand narratives, which sliced up the past for example into antique or classical and medieval, medieval and early modern, pre-industrialised

and industrialised, and so on (Tipps 1973; Lake 1998; Lorenz 2006; Nelson 2007). Thus, ‘early modernists are far too ignorant about the nature of late medieval society and politics’ (Lake 1998, 279), and modernisation, while popular for evoking ‘vague and generalised images which serve to summarize all the various transformations of social life attendant upon the rise of industrialization and the nation-state in the late eighteenth and nineteenth centuries’ (Tipps 1973, 199), proves very hard to pin down; ‘the relationship of modernization theory to the future study of the transformations of human society is roughly analogous with that of the Greek classification of the elements to the periodic table of contemporary chemistry’ (Tipps 1973, 222, n14). Where to put boundaries, and how to characterise continuities and tipping points, is problematic and always provisional (Lake 1998, 280–1; Nelson 2007, 198; cf. Bentley 1996, 749).

The timescale of Le Roy Ladurie’s *Montaillou* covers only 30 years. In a much later interview, Ladurie is reported to have classified historians of literature as ‘foxes or hedgehogs, butterflies or mole catchers, parachutists or truffle hunters’ (Le Roy Ladurie 1998; Hickey 2009, 507). Criticising Braudel, perhaps the greatest parachutist of them all, Paul Ricoeur, another leading advocate of narrative and emplotment (1984, 209, 224; and see also [Chapter 3](#)), commented that ‘geohistory is rapidly transformed into geopolitics ... global history never ceases to come ashore’ and ‘*all change enters the field of history as a quasi-event*’ (original italics). Microhistory, or *microstoria* in the Italian tradition with which it has often been associated since the 1970s onwards (Guldi and Armitage 2014, 11), shifts the focus to detailed, very short-term studies of particular contexts and groups (Levi 1991; Revel 1995). Carlo Ginzburg’s *The cheese and the worms* (1980) and Robert Darnton’s *The great cat massacre* (1984; cf. Arnold 2000, [chapter 6](#)) are good examples.¹¹ One recent formulation of the distinction between macrohistory and microhistory has been in terms of ‘prospect history’ and ‘refuge history’ (Brewer 2010). As John Brewer puts it (2010, 89), prospect history is ‘written from a single, superior point of view – a bird’s eye perspective or from a lofty peak – in which an extensive, large-scale landscape is surveyed and analysed ... Because of height, size and distance, what is observed and recorded is general not specific, an undifferentiated shape or aggregated trend whose contours and surface can be seen but which lacks distinct detail, though it may enjoy numerical expression’. Refuge history, on the other hand,

is close-up and on the small scale. Its emphasis is on a singular place rather than space, the careful delineation of particularities and details, a degree of enclosure. It depends upon the recognition that our understanding of what is seen depends on the

incorporation of many points of view rather than the use of a single dominant perspective ... historical figures are actors and have agency, motives, feeling and consciousness (Brewer 2010, 89).

We are back to the choice of scales; it is as though there is a perpetual, and at times undignified, scuffle to control the lens settings on the telescope and the microscope. The advantages of the close-ups of microhistory can be offset by lack of definition of their wider context and significance. One critic goes so far as to call microhistories ‘navel gazing’ (Vernon 2014, 132). Brewer (2010, 90) envisages (though he does not set out how this is to be done) an ideal amalgam of ‘grand narratives’ and ‘microhistories’. There has also been reaction recently to what has been called the Short Past (Guldi and Armitage 2014, 39). This is acknowledged as the currently dominant temporal trope among historians in general, and appears to be envisaged as studies covering a century or so, or decades (Guldi and Armitage 2014, 50, 59–60). Without seeking to set this aside, nor all the advantages of close detail, Jo Guldi and David Armitage (2014, 87) have called for much more attention to the Long Past, ‘the arena of long-term discussions of time’ and ‘the apprehension of multiple causality’, not least for the benefits which a long historical perspective might bring to the understanding of pressing modern problems such as climate change and social inequality, by countering short-term political responses and decisions. But their discussion of the specific spans of the long timescales which they seek to revive is disappointingly brief. It appears to cover anything from hundreds to thousands of years (Guldi and Armitage 2014, 20–8, 86).

The scales of anthropology

If anthropology overall has been concerned with the understanding of human nature and the study of cultural diversity (Barnard 2000, 180), it is no surprise that archaeologists have frequently turned to it, and social theory in general, as a fruitful source of ideas (Gosden 1999; Garrow and Yarrow 2010). The relationship has tended to be one-sided, and it has been claimed that some of anthropology’s relations are with the neighbouring disciplines of sociology, law, psychology and religious studies, through shared interest in society, culture, belief, gender and law (N. Thomas 1999, 262). One recent reflective account by an anthropologist looks at global culture as ethical imagination, through the lens of particular individuals (Moore 2011). The interaction has not been static, however; with the recent development of interest in relational ontology (as noted in [Chapter 1](#), and which I explore further in [Chapter 4](#)), the disciplinary boundaries are becoming less sharp. But there has been a tendency for

archaeologists, noted by John Robb (2007, 3), to try to apply to their data the latest good ethnography which they have read. I certainly see myself in the past as guilty of this charge.¹²

This brings two problems. First, it confronts us with the dilemma of whether to seek particular or generalised understandings, which the historian Gaddis (2002, 66) has underlined. Often it is not so much the latest single ethnography which is influential in archaeological interpretation as sets of ideas and approaches, frequently coming from a mix of sources in anthropology and social theory, and even theoretically minded social historians. In recent times, one can think of a long list which includes *habitus*, agency, practice, memory, personhood, the body, house societies, and most recently relational ontology and the turn to things. These and others are all important, and I will draw on most of them in one way or another in the course of this book. But over and over again we have tended to start with these big ideas, using them as a fixed or at least definable point of reference with which to approach remote pasts lacking written records. We run the risk of having made sense of things before we have started. This same tension has existed in anthropology as well (Moore 1999, note 1, 19–20; Knauff 1997), though it has been claimed that ‘the Holy Grail of grand Theory’ has now changed to a ‘softer theoreticism’, concerned with practice on the ground (Knauff 2006, 413).

In tending to use generalisations in this way, archaeologists are also, secondly, prolonging the atemporality of many past anthropological studies. Some time ago, the anthropologist Johannes Fabian (1983; cf. Fabian 2006, 143) critiqued the practice of distancing the Other as temporally remote, which he called ‘allochronism’. In a more recent account, the report is still that ‘most of social anthropology has been stuck in a synchronic quagmire’ (Barnard 2011, 147). Studies with temporal depth, such as the investigation, through an oral tradition, of the Tee ceremonial exchange cycle in the history of the Enga in Papua New Guinea (Wiessner and Tumu 1998), appear to be rare, just as studies with spatial breadth (such as Knauff 1993) also seem to have gone out of fashion. Perhaps the scale of close-contact, participant observation by anthropologists can be compared with that of microhistory, but its temporality often cannot. It is recognised that even notions of the flow of time are under-theorised, though future reconciliation with ‘multi-layered perspectives on history’ can be envisaged (Hodges 2008, 413; cf. Munn 1992; Gell 1992; Rockefeller 2011). But this may be unfair. Bruce Knauff (2006, 421) has argued that a series of studies have begun to unite anthropological and

historical approaches, 'through temporal frames longer than a few years but shorter than several centuries – typically between a quarter-century and a century and a half'. These scales resonate with what I will be exploring in this book.

For some, all this does not seem to matter. Tim Ingold (2010, 160) has envisaged a future in which archaeology and anthropology merge, archaeologists having 'dropped the pretence that what is past is any older, or more ancient, than the present, recognising that the occurrences of the past are not deposited at successive moments while time moves on, but are themselves constitutive of that very movement'. But though the breaking down of disciplinary boundaries is part of the argument of this book, and however we choose to attempt to describe the flow of life, succession should matter, sitting as it does at the heart of historical, interpretive narrative.

Archaeology as history?

Clive Gamble (2014, 160) has called for a fusion of history, archaeology and anthropology, under the banner of deep history. Such inter-disciplinary union would be highly welcome, but the weight of the components is likely to vary. In thinking about the possibility of tracing prehistoric forms of history, distinguishing between genealogical and mythical history, Chris Gosden and Gary Lock (1998, 5) saw history, as the outcome of human agency, as only possible with named figures and known acts. This view is why documents, even in fragmentary form, like the wonderful *Oxyrhynchus papyri* from Egypt (Parsons 2007) or the Amarna Letters (Moran 1992), are normally perceived as such powerful siren voices from the past.¹³ To write on early modern England, Keith Thomas (2009, 5) evokes a long list of contemporary sources, including books on theology and philosophy, letters, diaries, poems, plays, speeches, legal depositions and recorded conversations.

Clearly, the evidence for the Neolithic and Copper Age is very different, but I argue that if we think away from the differing nature of sources, written or material, and set aside the illusion that a written record somehow bestows complete understanding, the really fruitful future for an interpretive archaeology, theoretically informed by cross-reference to anthropology and social theory, is to think much more historically. Archaeology and history have much in common. Both disciplines select evidence and diverse themes from a varying mass of data. They can work at varying scales, as they conduct analysis and especially construct narratives of change, to make sense of the endless flow of life. I will go on to try to substantiate the claim that narratives at lifetime and generational timescales are now routinely within the

grasp of archaeologists, and that in that sense we should abandon the notion of prehistory, as several others have already called for. Such a framework should further help us to think at multiple scales, though how and if we can successfully do that is a question to follow in the rest of this book. I have argued above that we can work at finer scales more often than we realise. We are certainly familiar with working at the long term, and I do not want to lose that perspective. But we have the opportunity now to unpick the long term and examine its constituents much more closely. And when we come to re-assemble the long term, will it look the same through the lens as it did before?

Notes

1 Hooke had devised laws of elasticity, been the first to apply the term cell to the biological structure, worked on ideas of gravity, sought to determine how the respiratory system worked, developed a theory of how memory operated, and taken an interest in architecture and surveying, which was to lead to his helping Wren to design the dome on St. Paul's Cathedral.

2 Such as the new European X-ray Free Electron Laser.

3 A metaphor I owe to Fraser Sturt.

4 I offer my reflections on timescales in a kind of earth time, choosing to set aside relativity here, according to which the Ice Man, say, if he spent significant time in the mountains, would have been slightly older than a twin living down at sea level (Rovelli 2014, 8).

5 Though of course both grandparent and grandchild could be seen as modern constructs, a point I owe to Douglass Bailey.

6 'But if Miss Golightly remained unconscious of my existence, except as a doorbell convenience, I became, through the summer, rather an authority on hers. I discovered, from observing the trash-basket outside her door, that her regular reading consisted of tabloids and travel folders and astrological charts; that she smoked an esoteric cigarette called Picayunes; survived on cottage cheese and melba toast ...'

7 For more of the archaeology, see Birch (2012) and Birch and Williamson (2013).

8 See Notes and News in *Antiquity* 45, 1971, 130–4. And this was a time when the journal ran essays on 'Whither archaeology?', and soon after, a notable debate between David Clarke and Christopher Hawkes.

9 Evans (1997, 163) lists 'social history, quantitative history, microhistory, cultural history, women's history, gay history, black history, psychohistory, and so on'. See also Kelley and Smith (2012).

10 Founded in 2006.

11 But beware the warning from Richard Evans (1997, 247) that the narrative of the 'great cat massacre' was based on a single, three-page pamphlet written some 30 years after the 'incident' it purports to describe.

12 See for example my 2013 paper on enclosures, which directly borrows from Fredrik Barth's *Cosmologies in the making* (1987).

13 Moses Finley (1986, 45) stressed that it is groups of documents in the study of ancient history that begin to be informative, rather than single items.

Chapter 3

The rags of time: building chronologies

Narrative, plot, story: scale and tempo

When John Donne, in *The Sun Rising*, evoked the ‘hours, days, months, which are the rags of time’, it was to declare the timelessness of love – from a 17th-century poet’s perspective. How many such subdivisions of time should the historian need? At the start of his *Early Mediterranean Village*, John Robb (2007, 22) reflects on the consequences of the over-zealous collection of detail, using the imaginary case of the man who collected information only about the 1 September, 1973. He is in good company. Ferdinand Braudel repeatedly lamented the confusing quality of the short term, seeing it, for example, as a ‘surface disturbance, the waves stirred up by the powerful movement of tides. A history of short, sharp, nervous vibrations’ (Braudel 1980, 3). And ‘we must beware of that history that still simmers with the passions of the contemporaries who felt it, described it, lived it, to the rhythm of their brief lives, lives as brief as are our own’ (Braudel 1980, 4). That theme continues today, as seen in the title of Peter Hennessy’s book *Distilling the Frenzy* (2012) (adapted from a phrase of John Maynard Keynes), on making sense of contemporary events.

The resolution of this dilemma must lie in the kind of narrative that the historian chooses to create. Braudel advocated the medium and especially the long term, but just as historians can focus on individuals as well as broad processes, it also seems perfectly legitimate to concentrate, depending on the goal of enquiry, on specific days; the first day of the Battle of the Somme, for example, sprang to mind as I began to draft this chapter in the summer of 2016. There are of course plenty of books on just that, and on the months that followed, just as there are many others which put that particular struggle into context, and in turn seek to understand the chain of processes and events which led to war in the first place (Cabanès 2016; Stevenson 2004). But for an archaeology which seeks to create much more detailed accounts of human action and lives in remote pasts, shorn of the rich written archives, first-hand accounts and memoirs, photographs and

film of 20th-century history, it is sobering to reflect that there has been comparatively little reflection in the discipline on the nature of narrative.

This is not so in other disciplines. There are shelves in my university library full of books on narrative, in history and literature. I have already referred (in [Chapter 2](#)) to the discussion by Hayden White and others of the kinds of narrative employed by historians. Over 20 years ago, that approach was picked up by Elisabeth Rudebeck (1996) in a pioneering study of the narrative styles within which the transition to agriculture had been written about. Despite the phase when material culture was discussed as if it were some kind of text, for example, in Hodder (1989), this was not much further discussed. More recently, however, in the spirit of Hayden White, Richard Lesure (2015) has proposed that site monographs can often be characterised as a form of epic, to form a fifth mode of narrative emplotment, alongside the secondary syntheses which archaeology shares with history. This is on the grounds of the lengthy treatments, diversity of evidence contained, and multiplicity of themes covered, which monographs regularly provide. The comments from other researchers published with the article record a number of reservations, chiefly that while a few monographs might be thought of as epic, most cannot, and Lesure's own judgement (1915, 61) on Rudebeck, that she covered themes much more specific to agricultural transitions than comedy and tragedy, seems to apply to his own treatment.

I have found the approach of Paul Ricoeur in *Time and Narrative* (1984; and see also Ricoeur 1980) rather more helpful, despite its often difficult Gallic style, even in translation (for example in the threefold character of *mimesis* or stages of representation, following Aristotle). While acknowledging Hayden White and others, Ricoeur offers the essentially simple hypothesis that 'historical events do not differ radically from the event framed by a plot' (1984, 208), and concentrates on the process of creating plot, rather than on seeking to force it into a limited number of predetermined categories. For him, plot pulls things together and makes sense of them; 'I see in the plots we invent the privileged means by which we re-configure our confused, informed and at the limit mute temporal experience' (Ricoeur 1984, xi). This is because the temporal character of human experience is central; 'the world unfolded by every narrative work is always a temporal world' (Ricoeur 1984, 3), and 'in the final analysis, narratives have acting and suffering as their theme' (Ricoeur 1984, 56). Further, 'emplotment brings together factors as heterogeneous as agents, goals, means, interactions, circumstances, unexpected results'

(Ricoeur 1984, 67). This is not the place to set out in detail his historiographical analysis of the previous eclipse of narrative or previous practitioners. His own position is neatly summed up in the claim (Ricoeur 1984, 178) that ‘if every narrative brings about a causal connection merely by reason of the operation of emplotment, this construction is already a victory over simple chronology and makes possible the distinction between history and chronicle’, and in the observation (Ricoeur 1984, 225) that ‘something happens to even the most stable structures. Something happens to them – in particular, they die out. That is why, despite his reticence, Braudel was unable to avoid ending his magnificent work with the description of a death, not, of course, the death of the Mediterranean but of Philip II’.¹

Another inspiring point of comparison is a set of reflections by Victor Turner (1980) on chronicles and stories, and the social dramas they portray among the Ndembu people of Uganda. Turner too acknowledges the work of Hayden White, but seeks to ground his own account in the specific conditions of his Ndembu case study. Chronicles are more matter of fact, while stories about social conflict and antagonisms have a recurrent pattern of development, from break to crisis, and on to redress and resolution (Turner 1980, 149). The audience is directly involved, and stories and gossip themselves are important constituents of the framing of behaviour.

What all such accounts tend to leave out is a sense of the varying tempo of change and the inter-weaving of different scales (cf. Sahlin 2005). Those serve to show what is at stake in the attempt to write more consciously historical accounts of remote pasts. The next step, which is the substance of this chapter, is to review critically the resolution of our currently available chronologies for Neolithic Europe, and so to confront the question of the kinds of narrative and plot which we can reasonably hope to construct.

There are grounds for both optimism and pessimism. There has been a long, well established tradition of building chronologies, which stretches back well over a century, using the rich material assemblages which characterise the sequence in many areas. I will look at examples of both dendrochronology, precise to the calendar year, and high-resolution date estimates in a probabilistic Bayesian framework. Both those can work to best effect when combined with more traditional sources of information, based on the material and site stratigraphies. On the other hand, I will suggest that there remains a lot of fundamental research to be done on chronology. When we began our ToTL project, it was in the belief that the structure of the chronological edifice of the European Neolithic was basically sound,

and that we could essentially contribute refinements of precision here and there. We hope that we have indeed done that, but our own and other studies also suggest that there is far more uncertainty than commonly acknowledged. Given the uneasy and varying mixture of typology of the material, especially of pottery, some seriation, vertical and horizontal stratigraphic information, and the informal, visual inspection of often very poorly selected radiocarbon samples, this is in fact hardly surprising. My book is as much a manifesto for future action as a demonstration of universally reliable time measurement.

I will look briefly in turn at dendrochronology, typology and seriation, and the Bayesian approach to the interpretation of radiocarbon dates. These should not be thought of as separate; I will stress how the Bayesian approach is most powerful when it incorporates other information.

Dendrochronology

Dendrochronological dates are not confined to the Neolithic settlements of the Alpine foreland. There are important results from the trackways of the Somerset Levels and some from central Ireland, for example (Coles and Coles 1986; Baillie 1995; Baillie and Brown 2002; Brunning and McDermott 2013), and from a scatter of wells across the distribution of the LBK (Koschik 1998; Stäuble 2010); there are isolated examples too, such as from the La Draga settlement in north-east Spain (Palomo *et al.* 2015) or the Alvastra platform in central Sweden (Malmer 2002; Browall 2011). But the great bulk of dendrochronological data relevant to the study of the European Neolithic comes from the countries around the fringes of the Alps (Pétrequin 2013), initiated in the late 1930s, and increasing in quantity and analytical sophistication since the 1980s and 1990s; it is on those that I will concentrate, using examples especially from northern and western Switzerland, south-west Germany and eastern France.

Other things being equal, dendrochronology works, on well known principles, which I will not rehearse here (Baillie 1995; English Heritage 1998; Stöckli 2009; see also B. Becker 1982; B. Becker *et al.* 1985). Oak, luckily the main structural timber chosen by Neolithic people, is the optimum species to analyse, others having less distinctive growth patterns, which are still being investigated (Bleicher 2009a). It is salutary to note that the much discussed short chronology of Arbon-Bleiche 3 on the Bodensee (Leuzinger 2000; Jacomet *et al.* 2004) has recently been under critical review. The published dates for its occupation between 3384 and 3370 BC² were based on the ring measurements of both oak and fir, but the sequences

for both prove to be problematic (Schweichel *et al.* 2017). Nonetheless, though its absolute dates are now up in the air, there seems no doubt about the relative chronology of the construction of the houses of the settlement (Schweichel *et al.* 2017, 64), and the short duration of the site stands.

There are now enormous quantities of data available for analysis, even though there appears to be a lack of inter-laboratory comparison (Billamboz 2005, 55) and it seems clear that a lot of data remain unpublished (often simply because of the practical challenges of dealing with big data on this scale) (Fig. 3.1). André Billamboz (2010, 83) has previously estimated that around 100,000 individual database entries now exist on material from excavations and small sondages in his study area, the Bodensee or Lake Constance and Upper Swabia in south-west Germany. About a quarter of these have been fully analysed. There are over 20,000 samples from the well known site Hornstaad-Hörnle I alone (of which more below), of which over 4400 have been measured (Billamboz 2008, 148). Considerable quantities of data are still being processed from the cluster of sites around the head of Lake Zürich, which together form one of the densest and best studied data sets from anywhere in Neolithic Europe, most recently from Mozartstrasse and Parkhaus-Opéra (Bleicher 2015; Bleicher and Burger 2015; see also Archäologisches Landesmuseum 2016).

In general, one can observe differences in the kinds of cumulative analysis made possible by different kinds of site and setting, so that this is a record which – despite the big data – is far from uniform. There are situations like the head of Lake Zürich, or the inlets at the top of the Bodensee, or the bay of Sutz-Lattrigen on Lake Biel, where numerous adjacent sites have been found, and some of these, like Sipplingen on the Bodensee, or both Zürich-Mozartstrasse and Zürich-Parkhaus-Opéra, have shown repeated occupations more or less on the same spot (Billamboz 2014a; Jacomet *et al.* 2016). Some of these can be disentangled by the stratigraphy but in other cases it is the large-scale analysis of timbers from the mass preserved which helps to establish these long but punctuated settlement histories (Bleicher 2015a; 2015b). In other cases, quite compact, short-lived occupations are known, such as Arbon-Bleiche 3,3 or Bad Buchau-Torwiesen in the Federsee area (Bleicher 2009a; 2009b), with varying configurations of neighbouring sites roundabout. Pfyn-Breitenloo in the Thurgau area south of the north-west Bodensee is a case in point of what appears to be a short-lived occupation of the very late 38th century cal BC, with a scatter of probably contemporary neighbours only a few kilometres distant (Leuzinger 2007).

Not only is the dendrochronological record therefore variable in site terms, but it is also patchy in its overall coverage. The south-west German regional curve currently spans the period 4224–2417 BC (Stöckli 2009, 17). This means that very early Neolithic sites in the Alpine foreland of Switzerland, such as Egolzwil 3, cannot currently be dendro-dated, and the position of that site has been established by only four radiocarbon dates, three on unidentified charcoal and only one on cereal (Stöckli 2009, 16). In addition, the amount of wood available for measurement fluctuates considerably over time, as changes in lake levels have led to increasing or decreasing chances of preservation for timbers of certain dates (Stöckli 2009, 16). This means that for the Bodensee area the 38th, the 35th, the 33rd, part of the 31st, and part of the 29th centuries BC, are poorly covered (Stöckli 2009, 17; Billamboz 1998). In some instances, this is the outcome of raised lake levels in poorer climatic conditions – which in turn has led to very specific hypotheses about short-term adaptation, including reverting to more hunting in the colder and wetter setting of the 37th–36th centuries BC (Schibler and Jacomet 2010, and references).

I have suggested elsewhere that this specific model is not without its problems (Whittle 2003, 88), and it also raises the wider issue of the relationship of wetland and dryland sites in the Alpine foreland. Were people confined to lake and marsh margins, where the best conditions of preservation are found and where research has understandably been concentrated since the mid-19th century? Or did they also settle more widely? We know from the evidence for clearance that there must have been use of the wider landscape beyond lake margins. There is good evidence for the movement of both people and animals, and newer isotopic studies are beginning to refine this considerably (Jacomet *et al.* 2016; Bogaard *et al.* 2017). While there are many sites along the edges of bigger and smaller lakes such as the Bodensee, Lake Zürich, Lake Biel and Lake Neuchâtel, sites are also well known in more ‘inland’ situations, such as Pfyn-Breitenloo and its neighbours, already mentioned, Niederwil, and various ones around the small Burgäschisee (Stöckli 2016, and references), which also reinforce a sense of much wider landscape exploitation. This whole question remains open to more focused research, but it deserves to be remembered in overall assessment of the quality of the chronological record for the Neolithic of the Alpine foreland.



Fig. 3.1: Preservation by waterlogging, the precondition for dendrochronology in the Alpine foreland. Top: Ehrenstein, Kr. Ulm, in the valley of the Blau, under excavation in 1960. Below left and right: Bad Buchau-Torwiesen II. Photos by kind permission of Renate Ebersbach and Landesamt für Denkmalpflege Baden-Württemberg; © Landesamt für Denkmalpflege Baden-Württemberg.

These limitations and reservations stated, the outcomes of dendrochronological analysis are deservedly celebrated. Above all, there is a long rollcall of impressively detailed and on the whole strikingly short site biographies, or at any rate, when reoccupation through time is involved, of brief individual episodes of settlement. Just four examples, in order of appearance, serve to underline this. Hornstaad-Hörnle IA (Fig. 3.2), by the shore of the north-west arm of the Bodensee, was started in 3917 BC, relatively early in the Neolithic sequence of the region, and in this and the following year, at least nine houses were laid out, quite close together but variously orientated (Billamboz 2006, 317, Abb. 32), perhaps indicating smaller groupings within the nascent settlement. In 3915 BC 16 more houses followed, forming a rough line on the lake side of the pioneers; again their layout is somewhat straggling, compared to many other such settlements, but with close set pairs visible. In 3912–3910 BC, came a

further burst of building, especially in 3912 and 3911 BC with over 20 houses; these both filled in the area already defined, and extended it (Billamboz 2006, 317). Disaster then struck in 3910 BC, probably in late summer or early autumn, on the basis of the charred evidence for the crop harvest (Styring *et al.* 2016), when fire destroyed almost the whole settlement. A little rebuilding took place the following year, until 3907 BC, but with just six or more houses, seemingly in pairs; final activity is indicated from 3906 to 3902 BC, but without further buildings definitively identified. Only 200 m along the shore, at least one building was also set up in Hornstaad-Hörnle III in 3909 BC. Over subsequent years, the wider lakeshore was re-occupied several times, for example on the same spot (Hornstaad-Hörnle IB) in the 36th century cal BC, and close by (Hornstaad-Hörnle II) already in the 39th century BC and later (Hornstaad-Hörnle V and VI, in the 32nd and 27th centuries BC).

On the basis of 31 dendro dates on timbers from six houses (Houses 1, 2, 4, 9, 14 and 16), all of which fall between 3708 and 3703 $\pm$ 1 BC, Pfyn-Breitenloo was a short-lived, single-phase settlement, with some 17 houses either side of a lane. Radially-split timbers from the same tree trunk are used in several houses, reinforcing the sense of contemporaneity (5, 9, 10, 11, 14; 5 and 6; 9 and 14) (Leuzinger 2007).⁴ For the most part, there appear to be no distinguishable sub-layers within the cultural layer, and no recognisable build-up of debris from the apparent 4–6 year occupation period. Settlement debris could have been scoured or mixed annually by rising and falling lake levels. This is suggested by pollen core profiles taken from the site, which show a single annual sequence of pollen preserved in the animal dung (Leuzinger 2007, 128–9).

The short duration of Arbon-Bleiche 3, even though its absolute date is now uncertain, could be broken down further, with the help of both the dendrochronological analysis and the stratigraphy of deposits in and around the houses, and the life of the site ended with a flood and, once again, a fire (Jacomet *et al.* 2004; Doppler 2013). This probably 34th-century BC settlement was closely packed, with 27 houses laid out neatly and cheek by jowl in at least nine rows parallel to the lake shore; the site was incompletely excavated (Leuzinger 2000) (Fig. 3.3). Eight construction phases were identified. The first house was located in the centre of the excavation. Two further houses were built in the immediate vicinity a year later. Two years further on, a building boom ensued, with ten new houses erected. Rows of houses now emerged, with alleyways providing access to both their long and short sides. A further row of houses was added lakewards

after another two years. Three further buildings were erected a year later, one unusually small. A further three houses were constructed the next year, on the fringes of the existing core; one of these, and others built in the two following years, began to form an additional row of houses on the lake side. No further houses were built after this point, at least in the excavated part of the settlement, with only repairs carried out on the already existing buildings. A devastating conflagration brought the settlement to an end.

Finally, Bad Buchau-Torwiesen II, by the Federsee in south-west Germany, is an even shorter-lived settlement, its houses either side of a lane spanning just 3283–3279 BC (Schlichtherle *et al.* 2011); it is one of a series of small sites in this region late in the Neolithic, nearly all of short duration (Bleicher 2009a; 2009b). Once again, one or two pioneer buildings were set up first, and the rest followed a year or two later, in a rush (see again [Fig. 3.1](#)). One of the surprises of many of such briefly occupied settlements has been the extent of contemporary diversity in activity, as evidenced at both Arbon and Torwiesen. Two of the four examples I have given came to an end through fire, and whether such burning was accidental or deliberate has been much debated. But it is worth stressing that there are plenty of other examples of settlements which appear to have been simply abandoned, long before their buildings might be supposed to have decayed from the effects of time and weather. In eastern France, around the small lakes of Chalain and Clairvaux, settlement lives of little more than ten years appear to have been the norm through the fourth millennium BC (Pétrequin 1997; 2013, and references). Nor is there any obvious overall or uniform trend towards increased site duration through the Neolithic sequence of the Alpine foreland as a whole (Hofmann *et al.* 2016).

Dendroarchaeological analysis also now includes what is called dendrotypology, developed especially on the wealth of samples available from the Bodensee region (Billamboz 2006; 2014a; 2014b). This seeks to sort timber by tree age, growth patterns and wood anatomy; it has been employed particularly on the many young timbers found. It relies on shorter sequences than the most rigorously established, longer site and regional sequences, and on visual matching (Billamboz 2014a, 1279), so is in part experimental and speculative. It has helped to provide a series of important insights into woodland management and landuse (see also Jacomet *et al.* 2016). Two prime examples are Hornstaad-Hörnle IA, again, and Sipplingen on the Obersee of the Bodensee (Billamboz 2006; 2014a). At the former, dendrotypology helped to show the use of woodland

regenerated by intensive coppicing, with timbers reflecting such landuse being found particularly in the building infilling of 3912–3910 BC (Billamboz 2006, 354). Sipplingen had an unusually long, though intermittent, occupation, from 3920 BC onwards. Dendrotypology combined with other evidence has served to sketch a long, detailed history of landuse, which helps – to select just a few features from a much longer list – to discriminate between use of the hinterland and use of the lakeshore zone, to detect coppicing and short-term woodland rotations, pioneering settlement followed by expansion, and in due course retrenchment, perhaps in part as a result of land overuse (Billamboz 2014a, 1279–85). All this is held within the precise frame provided by dendrochronology.

It is also worth noting, very briefly, the contribution of dendrochronological analysis to the study of climate. Plenty of researchers look to shifts in climate as prime movers in change, but trends are often derived from distant sources of evidence, including in the north Atlantic. In the Alpine foreland, there is much tighter chronological control, even though there remains vigorous debate about the effects of climate on human action (Magny *et al.* 2005; Magny 2013; Bleicher 2013).

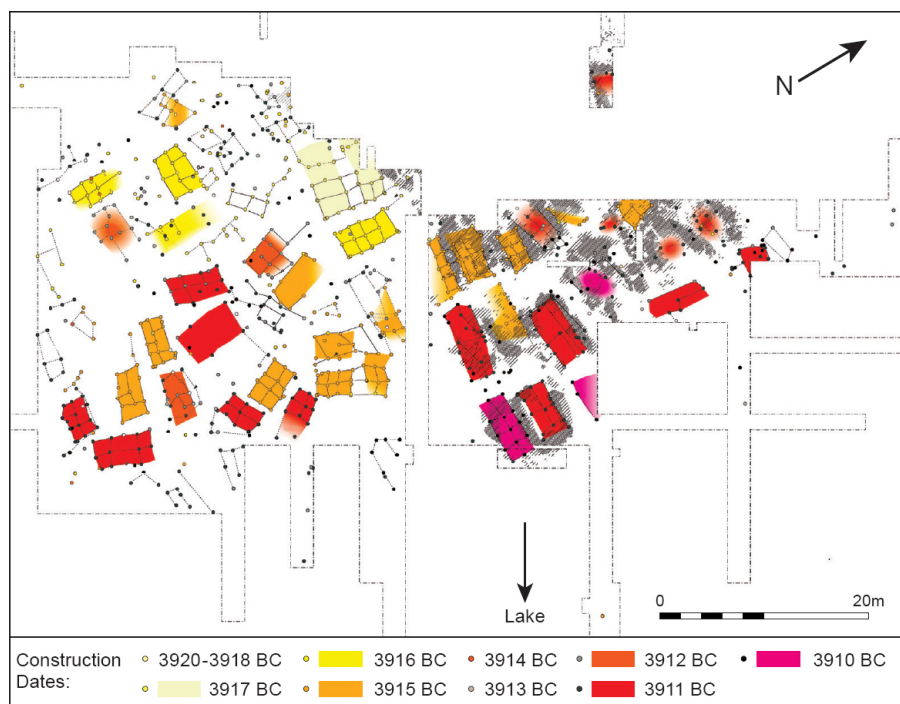


Fig. 3.2: Simplified plan with dendrochronological dates of the Hornstaad-Hörnle IA settlement, Bodensee; dated houses are shown in colours, and other settings of posts are indicated by dots. After

If history is to do with tracking detailed sequences of human action, here then is the basis for rich narratives of the fourth and third millennia BC, precise to the calendar year and even on occasion to the season. Despite the limitations and gaps noted above, this framework is not confined to exceptional single sites, but can be used across a reasonably wide area; and future research should extend our knowledge of the eastern and southern parts of the Alpine foreland. Given the often astonishing chronological precision now available for study, it is surprising in many ways that more researchers do not exploit this evidence, or weave it to a greater extent into their accounts of past lives several thousand years ago. Of course, there are pragmatic reasons why this does not happen. The sequence for the Alpine foreland region does not cover the whole span of the European Neolithic, and if there are opportunities here for the most chronologically detailed accounts imaginable, these are often narratives of a particular kind: stories of households and small communities going about their daily business of living and managing their immediate surroundings. This perhaps persuades many researchers to treat the Alpine foreland as a rather unusual kind of ‘reservation’. And on the other hand, the increasingly detailed site and specialist reports, and their sheer volume, make it difficult to master this kind of evidence. Related to that, even here, with chronological precision available to precise calendar years, we have to work at interpretation, to turn chronicle into wider history, to turn mere succession into narrative, and to trace the character of the plot or plots that result. And that is something that ‘prehistorians’ have not been used to attempting.

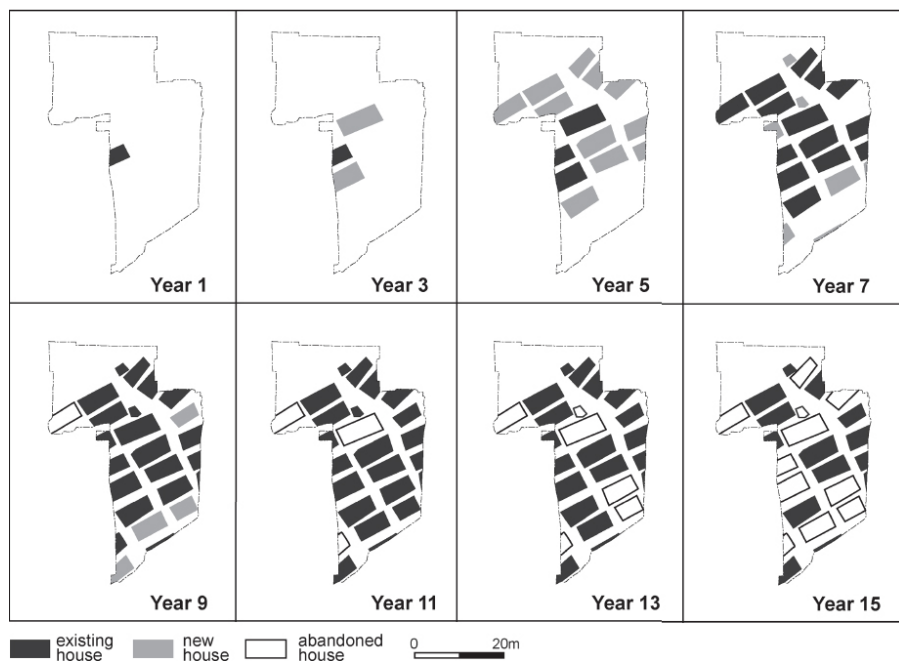


Fig. 3.3: The sequence of house construction in the settlement of Arbon-Bleiche 3. After Leuzinger (2000); Doppler (2013); and Schweichel et al. (2017).

Although this book is not dedicated to the Alpine foreland, two brief examples may help to show what difference having such time resolution makes to the kinds of narratives we choose to write. First, there is the sphere of settlement, economy and landscape: the active taskscape. Examples already noted such as Hornstaad-Hörnle IA, Arbon-Bleiche 3 and Bad Buchau-Torwiesen give vivid snapshots of activity. At the site scale, with high-resolution chronological control, the impression of diversity between houses and groups of houses is striking.

At Arbon-Bleiche 3 (Leuzinger 2000; Jacomet *et al.* 2004), it seems that particular groups of houses may have specialised in specific tasks. Here this is clearer in relation to animals; each house probably processed its own supply of cereals, as each house had a quern. Houses 1–4, 8, 20 and 24 stand out for having more than 50 percent wild animals in the bone assemblages from them and their immediate vicinity; houses 7 and 23, on the other hand, yielded extremely small amounts of wild animals and consequently the highest proportions of domestic animals. While domestic cattle bones were dominant in the northern part of the village, domestic pigs were more frequent in the southern part of the village. The largest amounts of deer bones were recovered from houses 3, 8, 20 and 24. Wild cattle were very frequent in houses 3, 8 and 20, while houses 8 and 20 had additional

concentrations of brown bear, marten/polecat, badger and otter bones. In the northern, landward area of the village, pike, perch and particularly fish of the carp family dominated. These were probably caught in set nets close to the shore, which was also confirmed by the greater frequency of net sinkers found in this area. In contrast, whitefish dominated in the southern, lakeward part of the village. These had to be caught from boats on open water, probably using trawl nets. The occupants of the northern half of the settlement were responsible primarily for cattle husbandry and nearshore fishing, whereas those living in the southern, more lakeward half of the village mainly kept pigs and had specialised in fishing for whitefish. Sheep and goats were kept both in the northern and the southern halves of the site; however, only in certain houses were they present in larger numbers.

At Hornstaad-Hörnle IA there is evidence in the range of tools that each house was an autonomous unit, but there are also pairings and short rows of houses (Styring *et al.* 2016, 98). Detailed investigation of the crop remains preserved in the fire of 3910 BC, especially crop isotope analysis, suggests that different houses cultivated, including through manuring, wheats with greater and lesser intensity, although there was apparently more uniformity in barley cultivation, which leaves open the possibility of more communal production of that crop (Styring *et al.* 2016, 101–2).

That kind of analysis ties in with the broader model that cultivation probably took place in fixed rather than shifting plots (Jacomet *et al.* 2016; Bogaard *et al.* 2017). Any one situation at both site and regional scale may have been complex, as already suggested by dendrotypology. There is evidence for the grazing of stubble and fallow fields, the tending of hedges or stands of fruit trees, burning out in the woods, perhaps as part of grazing strategies, the keeping of at least some herds away from settlements at certain times of the year, and hunting (Jacomet *et al.* 2016). There is also evidence for cycles of landuse (Jacomet *et al.* 2016; Bleicher 2009; Billamboz 2014a). So the snapshot pictures of particular households at work in specific settlements have to be combined with not only a wider taskscape but also varying timescales. Our own suggestion (Hofmann *et al.* 2016) was to integrate a sense of the flexibility and fluidity of household and settlement identity (Ebersbach 2013; D. Hofmann 2013) with a longer-term commitment to particular parts of the landscape. People could come and go, witnessed in part by the pioneer founders of settlements, and individual locations would be regularly abandoned, while plots, hedges, fruit stands, grazing, managed woods and hunting

grounds would see much longer-term continuity. There is also a much longer-term history of change, with a gradual shift to slightly more open landscapes in some locations, such as around the head of Lake Zürich by the Horgen period of the later fourth millennium BC, though this is not evident everywhere (Bleicher 2009a). It is the combination of timescales which helps to unlock the distinctive character of the Alpine foreland Neolithic lifestyle.

In the same way, high-resolution chronology also reveals more of the ways in which material culture was used to express and negotiate identity at varying scales (Ebersbach *et al.* 2017). This was a connected world, and studies of sources of stone tools show preferences that could shift rapidly through time (Pétrequin 1993). Most of the pottery through the sequence had rather simple forms and was quite plain; there are very few decorative elements (Stöckli 2009). The differences between discernible regional traditions were not extreme, but on the whole rather subtle. Dendrochronology allows change to be tracked with unusual resolution. This shows, for example, the slow spread of Pfyn pottery from the Bodensee/Lake Constance to the south and south-west, reaching Lake Zurich in the 38th century and Lake Zug in the 37th century BC. Traditional concepts of grouping similar objects into archaeological cultures are for the most part rather unsatisfying. The widely shared elements in material culture, house architecture and burial customs (the latter largely invisible) may be one manifestation of a social strategy of fluid allegiances, picking up and using influences from close and even more distant neighbours without fundamentally changing one's own traditions. That can also be suggested again from the snapshot pictures, for example at Arbon-Bleiche and Bad Buchau-Torwiesen, where there is diversity in material, including 'exotic' Boleráz pottery at Arbon better known from the eastern end of the Alps (de Capitani *et al.* 2002); individual houses or house groupings with varying material identities could nonetheless be integrated into functioning and close-knit settlements. That also comes through in the study of Concise sous Colachoz on Lake Neuchâtel (Burri 2007; 2009). There, the changing repertoire of pottery through several well dated layers of the earlier fourth millennium BC shows a combination of local (Cortaillod) and non-local styles. The 'exotic' material belongs to the style of the Néolithique Moyen Bourguignon (NMB) from the western side of the Jura hills. Successive migrant NMB potters have been suggested (although one could also think of imported pots), but local style persisted.

There are also situations in which much more rapid change can be

followed, particularly in later parts of the sequence. Pierre Pétrequin (1997, 55–6; see also Pétrequin 2005; Kaeser 2006) has related a rapid turnover of new pottery styles (Horgen and Ferrières) in the various Chalain and Clairvaux sites in the late fourth millennium BC to successive incomers; he further correlates this with local falls in the amount of deer hunting, which picks up again after integration (Pétrequin 1997, 56). Whether the appearance of Corded Ware in eastern Switzerland is due to the arrival of new people is open to question (and see [Chapter 6](#)), since there is plenty of continuity in most other aspects, but the ceramic shift seems rapid, in the 28th century BC (summarised in Ebersbach *et al.* 2016). In western Switzerland at this time, however, local traditions carried on side by side with Corded Ware pottery. Elements of Corded Ware pottery are ‘translated’ into something mostly resembling local pots, but with some Corded Ware style decoration or with a flat instead of a curved base (Stöckli 2009, 114, Abb. 68). Once again, multiple timescales help us better to unpick the flow of daily life and the tempo and character of change.

Typology and seriation: floating sequences?

As already noted, dendrochronological dating is largely confined to the Alpine foreland, as discussed above. There are some dendrochronological dates from LBK wells, which provide important checks for the absolute chronology for the later LBK (Koschik 1998; Gronenborn 2007a). Michelsberg and related pottery of the earlier fourth millennium cal BC, which is normally ordered and dated by conventional means across its broad distribution, can also be fixed by dendrochronological dates from sites around the Bodensee/Lake Constance (Matuschik 2010; 2011); the same can be said of course for Corded Ware in Switzerland (Stöckli 2009; 2016). These links, however, are rare. Elsewhere the chronological framework for the European Neolithic has been built up largely by a combination of typology and site sequences, reinforced over the last decades by radiocarbon dating (Stöckli 2006; Schier 2014); there are also some seriations of the material ([Fig. 3.4](#)). If this practice often seems taken for granted, it is in part because the tradition of such chronology building goes so far back in the history of research, on the one hand to foundational thinkers such as Oscar Montelius (Stöckli 2006; Stig Sørensen 2015, 86) and Flinders Petrie, and on the other hand to heroic figures on the ground at the start of it all, like Jaroslav Palliardi (who started in law) in Moravia in the 1880s and 1890s, who worked in gravel pits and construction sites (Kovárník 2008), and the Jena historian Friedrich Klopffleisch, who was among the first to order LBK

material (Neumann 1932) (Fig. 3.5). In megalithic studies, there are comparable pioneers; to take just southern Iberia, already in the middle part of the 19th century there were the first excavations and discoveries, as at Menga and La Pastora (Mitjana 1847; Tubino y Oliva 1868), followed shortly by the first syntheses (Tubino y Oliva 1876; de Góngora y Martínez 1876). There are many other such examples, region by region across Europe. It is instructive to look back at some of the early struggles to get the sequence right. It took quite a while, for example, for the early position of what is now called the Körös culture correctly to be established, from the later 1930s onwards (Kutzián 1944; Trogmayer 1967), but that was begun well before the introduction of radiocarbon dating. But for the most part, the relative sequence began to be teased out relatively swiftly. Another example is the chronological table given by Ferenc Tompa (1929, 64) for central and eastern Europe, using building blocks (including Bandkeramik, Sesklo, Dimini, Tripolye, Cucuteni and others) which are still familiar, even though the suggested absolute chronology was far too low.

Year (cal BC)	Great Hungarian Plain						
	Serbia	Transdanubia	Middle & Lower Tisza	Upper Tisza	Eastern Plain	Transylvania	Bulgaria
3500	Bubanj-Hum	Balaton-Lasinja	B Bodrogkeresztúr A	B Bodrogkeresztúr A	B Bodrogkeresztúr A	Pécska Bodrogkeresztúr A	?
4000	Vinča D2	Lengyel III	B Tiszapolgár A	B Tiszapolgár A	B Tiszapolgár A	B Tiszapolgár A	Karanovo VI (Gumelnița)
4500	Vinča D2	Lengyel III	Proto-Tiszapolgár	Proto-Tiszapolgár	Proto-Tiszapolgár	Proto-Tiszapolgár Petrești, Erősd	Karanovo VI
5000	Vinča D1	Lengyel II	Tisza III	Csöszhalom (Óborin)	Herpály III	Petrești	Karanovo V
	Vinča C	Lengyel I	Tisza II		Herpály I-II		(Marica)
	Sopot-Bicske II		Tisza I/II	Tisza I/II	Tisza I/II	Lumea Noua	
5500	Vinča B2	Sopot-Bicske	Tisza I	Szakálhát	Esztár	Precucuteni I-II	Karanovo IV
	Vinča B1	Zseliz- Notenkopf DVK	Szakálhát	Bükk-Szilmeg AVK			
	Vinča A		AVK Körös IV		AVK	Criș IV	Karanovo III
6000	Starčevo III-IV	Starčevo III	Körös III	Körös - Szatmár	Körös - Szatmár	Criș III	Karanovo II
	Starčevo II	Starčevo II	Körös II			Criș II	
	Starčevo I	Starčevo I?	Körös I			Criș I	Karanovo I

Fig. 3.4: A typical example of a chronological chart, here illustrating the sequence on the Great Hungarian Plain and in surrounding key regions. After Parkinson (2006).

In the case of the Vinča culture of the central Balkans (Tasić *et al.* 2016a, and references), a key role was played by the name-giving site, the great tell of Vinča-Belo Brdo on the Danube just south of Belgrade. Material started coming to notice in 1904, and excavations started in 1908, led by Miloje Vasić of the National Museum, the first professional archaeologist of Serbia, born in Gradište and educated in

Berlin and Munich. His fieldwork was to continue at intervals until 1934. A section was dug through the c. 8 m Neolithic deposit of the tell, not counting another metre and more of overlying Copper and Bronze Age, and early medieval deposits. The work was recorded by fine glass plate photography, and finds logged by depth, within a site grid. In a manner prefiguring continuing modern practice, a wide range of funding sources supported the work, including no less than the Russian Imperial Archaeological Institute in Istanbul, and later on Sir Charles Hyde, then editor of the *Birmingham Evening Post*, millionaire, race horse owner and flamboyant social figure, who supported the Belo Brdo excavations in 1930–34; his help was reported in the *Illustrated London News* for 18 October and 1 November 1930. One significant pot was tactfully dubbed the Hyde Vase. Numerous interim reports were published, bringing the site to international attention, and Vasić promptly published four volumes on the site, *Preistoriska Vinča*, in 1932 and 1936.



Fig. 3.5: Early heroes of the effort to build chronology for the European Neolithic. Clockwise from top left: Friedrich Klopffleisch; Francisco Tubino y Oliva; János Banner; and Jaroslav Palliardi (Archiv of the Vysočina Museum in Třebíč).

For reasons that are not entirely clear, Vasić ended by getting the chronology spectacularly wrong. Initially (Vasić 1906) he assumed that the Vinča deposit formed between the Neolithic and Hallstatt periods. Later, he paralleled the Vinča material with the horizons of Troy (Vasić 1932, 87), and soon after he placed Vinča in the second half of the 7th century BC, as some kind of Aegean colony (Vasić 1936, V, VI, 153). That now perhaps seems harmlessly quirky, but the important point for this discussion is that his contemporaries were already highly critical, including Vladimir Fewkes (1935), who worked in the Balkans at this time for the then American School of Prehistoric Research, and had excavated the early Neolithic site of Starčevo across the Danube not far from Vinča in the early 1930s.

From a quite early date, Vinča was recognised as the principal (though not only) yardstick against which other sites in its network have to be compared, and was to serve as a key link between other Neolithic worlds to its south and north (among many others, see Childe 1929; Milojević 1949a; Garašanin 1979; J. Chapman 1981; Parzinger 1993). In terms of chronology building, this was part of culture history, or tracking the sequences of the archaeological assemblages thought at the time to represent cultures. The actual term ‘Vinča culture’ was probably first formalised soon after the second world war, for example by Vladimir Milojević (1949a; 1949b). In *The Danube in Prehistory*, Gordon Childe (1929) had simply referred to ‘the first Neolithic civilization on the Middle Danube’ and ‘the civilizations of period II’, and defined both Vinča I and Vinča II phases by the material from Vinča-Belo Brdo itself, which he had visited and discussed with Vasić, with some cross-reference to other sites. For Childe, Vinča-Belo Brdo was one of the key points for confirmation of his idea of a gradual spread of the Neolithic from the Aegean.

Vinča has rich material, including pottery, figurines, clay altars, bone tools and lithics, but it was the pottery above all which was the subject of classification (Fig. 3.6). This is best known for its black-burnished vessels, but there are other finishes, and Childe (1929, 27–8) already gave attention to varied surface treatment, fabrics and red slipping. There are many forms, including various kinds of bowl, bowls with tripod feet, pedestalled vessels, bowls and dishes with protomes (or projections from the rim), amphorae of various kinds and sizes, some with accompanying lids (some of distinctive prosopomorphic or face-like form), handled jugs, miniature vessels, and plates with thickened rims. The overall quality, compared to say the beaten-up appearance of quite a lot of British Neolithic pottery, is impressive. Childe’s overall classification was merely twofold, but it set in motion a long process of refinement which is still continuing today (Tasić *et al.* 2016a and references).

Though this does not necessarily speak to every kind of typological classification, it tells us a lot about the process. In this case, there has been for the most part what I would call an informal and impressionistic typology, based on what successive researchers could establish from what they could see of the material; the sheer quantity and range of the material have to be taken into account. As a result, no hard and fast or completely comprehensive typology has been established for the whole, vast ceramic assemblage from Vinča-Belo Brdo. What we have are a successive series of schemes – partly complementary, but partly competitive – characterising the

assemblage as a whole, at chosen depths through the stratigraphy of the tell ([Fig. 3.7](#)). This establishes general trends. So, for example, simple conical and globular bowls occur early accompanied by carinated bowls whilst more everted as well as inturned rims are late, as are tripod bowls. Pedestalled vessels do not occur above 4 m relative depth, their bases becoming progressively more solid. Lids, altars and miniature vessels are also mainly early, while plates with thickened rims, S-shaped profiles and handled jugs are late. While much of the pottery is plain, making its impression through high-quality surfaces, there is a range of decorations, and these too change through time. Thus, incised linear or zigzag bands with infilling punctuations are one typical form of decoration, found especially on bowls and lids in earlier levels. Grooving and channelling were also used, in linear arrangements in earlier levels, and curvilinear ones later, and then in linear fashion again towards the end of the sequence. Some early painted wares are known, principally bowls with red on black linear decoration, seemingly replaced by burnished treatments in later levels; there are also late red-painted or 'crusted' wares.

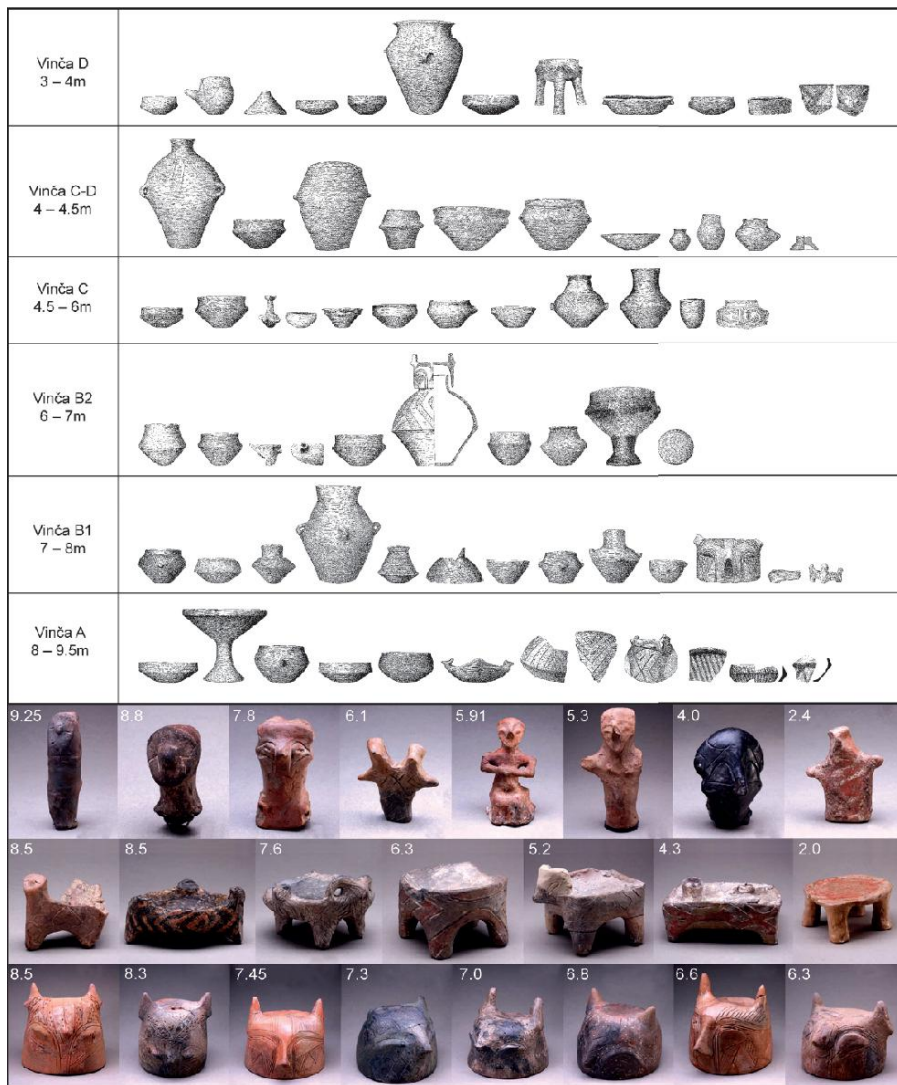


Fig. 3.6: A selection of Vinča culture material from Vinča-Belo Brdo. The numbers on the lower three rows indicate depth (in metres) from the surface of the tell. After Tasić et al. (2016a).

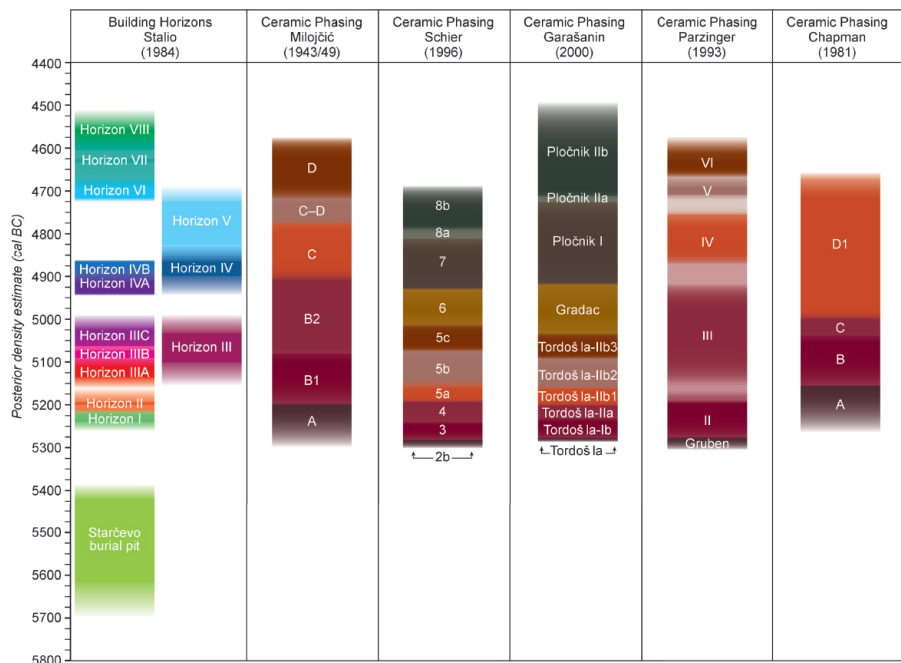


Fig. 3.7: Selected schemes for the periodisation of the Vinča culture, with modelled timescale. After Schier (1996) and Tasić et al. (2016b).

This process of classificatory refinement and replacement began with the Austrian scholar Oswald Menghin (1931), and was then enhanced by the German prehistorian Friedrich Holste (1939) (elected to the Marburg chair just before his death in 1942), whose division of the material by typology into successive phases, A–E, by position in the stratigraphy, set the pattern for all subsequent research; his A–D cover the Neolithic development of the site. In 1949 Vladimir Miložčić, basically using Vasić’s *Preistoriska Vinča*, added sub-phases B1 and B2. His phase B1 denotes a time when meanders still appear as a decorative motif and his phase B2 is the time of appearance of curvilinear motifs which were more visible in the following phase, Vinča C. Miložčić also identified a transitional C–D phase between 4.5–4 m, marking the transition to the last Neolithic phase at Vinča (Miložčić 1949a, 266–7). He was also the first fully to incorporate other Late Neolithic sites from the central Balkans into his chronological system. A shorter account published in the same year (Miložčić 1949b) shows the broad classifications used at this time. Milutin Garašanin (1951) was the first to make an inventory of the entire pottery collection from Vasić’s excavations at Vinča. He systematised the material from Belo Brdo and divided it into two major phases, Vinča-Tordoš and Vinča-Pločnik, drawing also on material from other sites in the wider Vinča culture distribution.

Hence, he used the labels Tordoš and Pločnik to link his early and late phases of the Vinča culture (as a whole) with the northernmost and southernmost sites known at the time. He sub-divided both his Vinča-Tordoš and Vinča-Pločnik phases, and later elaborated this division even further (Garašanin 1979; 1993; 1998; 2000). Hermann Parzinger (1993) was the first to provide quantified estimates of the varying occurrence of selective decorative motifs and forms by depth through the Belo Brdo tell, which he incorporated in a comparative scheme of successive cultural horizons, which covers the wider west Balkan region, and indeed far beyond; this was significant (though it deliberately eschewed the help of radiocarbon dating), but the actual treatment of Belo Brdo forms a comparatively small part of a very big volume. Meanwhile there had been other versions, such as by John Chapman (1981), and other schemas more concerned with the wider Vinča culture (of which more in [Chapter 4](#)), such as those by Berciu (1961), Dimitrijević (1974) and Lazarovici (1981).

A further step-change is represented by the analysis of Wolfram Schier. This established eight main phases and several sub-phases and covers the sequence from the beginning of the Vinča occupation on the tell to 4 m from the top (Schier 1995; 1996; 2000); further work was interrupted by the Balkan conflicts of 1998. The phasing is based on detailed analyses of changes in the form and decoration of bowls, since these seem to be the most chronologically sensitive form. So Schier defined 180 formal types organised into 23 type groups (2000, 189), classing them, broadly speaking, into conical, biconical, globular, carinated and tripartite, with variation also in their rims. Chronological credibility was tested by correspondence analysis: of 204 pottery types and decorative elements from 39 stratigraphic units (10 cm levels and pits from the lowest horizon). That in turn was supported by initial radiocarbon dating, later amplified by Dušan Borić (2009), and subsequently greatly extended by Tasić *et al.* (2016a). Schier's work has been fundamental, but it also serves to show how much more analysis there remains to do.

I have given such detail about Vinča-Belo Brdo because it illustrates so much of the process of how typological research has developed. While there have been plenty of significant studies of for example stone tools, such as of perforated axes and hammer-axes in central-northern Europe (Brandt 1967; cf. Davis and Edmonds 2011), and more recently, the stunning analysis of both the Alpine sources and typo-chronological development of jadeitite axes across much of Europe (Pétrequin *et al.* 2012), there is no doubt that the dominant material in the chronological edifice of the European Neolithic has

been pottery. The narratives constructed on the basis of the kind of typochronological classifications constructed for Vinča and elsewhere have for the most part been firmly within the culture-historical approach. One can remember here the vivid picture suggested for the moment when pots are created in the potter's hands (Gosden and Malafouris 2015, 704–7), and the possibilities this would appear to raise for variation, individual expression and change, but the culture-historical approach has broadly assumed that a much more collective, and often more conservative, identity is being expressed through pot form and decoration.

With ethnoarchaeological investigations which suggest all manner of social strategies being worked out through material culture (Hodder 1982), and now scientific analysis of former pot contents, through preserved lipids or fatty acids (Evershed 2007), it is easy perhaps to set this older, typological approach aside as oldfashioned (Sørensen 2015, 90). The real culprit here, however, has been the unthinking equation of material assemblages with unproblematic group identity, rather than the application of typological analysis itself. As I will argue later, there is anyway a strong case for rethinking many questions of identity in the light of new aDNA research, and there is every reason to wish for more rather than less typological analysis. As Marie Louise Stig Sørensen has pointed out (2015, 90–1; cf. Lucas 2015, 4), typology as the registration of object-similarities is important for the 'the human-object intersection' and for the analysis of change. But if typologies are 'also automatically about time' (Sørensen 2015, 90), the question is how good traditional or conventional time control has been. And the clear answer must be that there is enormous scope for improvement and refinement.

An important first step is to reflect on the spectrum of pottery typologies. Such comparison is, I believe, rare, since typology is so much taken for granted as part of basic practice. Approaches range from more impressionistic and generalising to highly detailed and formal; some emphasise forms overall, while others proceed by first looking closely at individual attributes, and then building up types and the differences between them. In Britain, for example, rather broad categories of pottery have been suggested, starting with Carinated Bowl at the beginning of the sequence. Its analysis goes back to a single, broad-brush paper (Herne 1988); in Scotland, some refinements have been added with the notion of Modified Carinated Bowl (Henshall 1972; Sheridan 2016). There is still no detailed typology for developed bowl pottery in southern Britain, and it has been tempting to fall back on ideas of a mixture of forms and

decoration, finding different expression at individual sites (Cleal 2004). Our understanding of middle Neolithic Peterborough wares in Britain still relies on the unpublished PhD of Isobel Smith (1956), while the comprehensive analysis of late Neolithic Grooved Ware has made little progress beyond the broad transregional styles suggested on the basis of form and decoration by Ian Longworth (Wainwright and Longworth 1971), though supplemented by further regional studies (Cleal and MacSween 1999; MacSween 2007; MacSween *et al.* 2015; Richards *et al.* 2016; Sheridan *et al.* in prep.). This can be contrasted with the much more rigorous analysis of form introduced for Michelsberg pottery by Jens Lüning (1967), which radically upgraded a previously less formal and more impressionistic approach (e.g. Scollar 1959). That laid the basis for a fivefold phasing of the Michelsberg culture, which has been the subject of much discussion and refinement over the decades since, but which has basically stood the test of time. Cross-checks provided by dendrochronology have already been noted, and seriation (of which more below) has also been applied to selected phases (Höhn 2002).⁵ Detailed formal analysis of southern Chasséen pottery can also be noted (for example, Sargiano *et al.* 2010; Borrello and van Willingen 2012). In contrast, it seems fair to say that analysis of TRB pottery remains a work in progress (Müller *et al.* 2012; Müller and Peterson 2015). There is a generally agreed sequence for Danish Neolithic pottery (see Müller *et al.* 2012, Abb. 1), and some sense of regional variation, but again perhaps the sheer quantity of material, and the plethora of decoration, have inhibited more recent analysis.

One can point to other examples where there has been no lack of intense interrogation. For the Hungarian sequence, the late Istvan Zalai-Gaál (Zalai-Gaál *et al.* 2014; Zalai-Gaál 2016) pioneered an exhaustive breakdown of formal attributes for the analysis of both Lengyel and Tiszapolgár/Bodrogkeresztúr pottery, principally from closed contexts in graves, which is the basis for the definition of multiple types and detailed phasing, and in turn for seriation (cf. Diaconescu 2014). Probably the most widely applied detailed typological analysis is that of LBK pottery, more or less across its whole distribution (usefully summarised in Pechtl 2015). This does not ignore form (nor even – so often missing from purely typological studies – a sense of the ceramic inventory of households: Pechtl 2015, fig. 29.4) but has very much concentrated on the classic ‘band’ decoration found on smaller and larger bowls and necked jars. Band decoration can be looked at as both individual motifs and overall schemes. Both may in part project individual household or other

identities, and so difference might not only be related to time, but for the most part, the profusion of decoration appears to be very time-sensitive, and is the basis for the plethora of detailed regional phasings in the literature. This is not the place to cite them all (see Bickle and Whittle 2013; D. Hofmann in prep.); good examples come from the study of LBK settlements in eastern Austria (Lenneis and Lüning 2001; Lenneis 2010) and the Aldenhovener Platte in north-west Germany (Stehli 1994), where up to 14 phases have been suggested. No less than 17 house generations have been proposed for the development of the Vaihingen settlement in south-west Germany, and the ceramic discrimination is at the heart of a complex story of household and clan history (Strien 2005; Bogaard *et al.* 2011). In both these cases, the extremely detailed typology of decoration has been the basis for seriation.

Seriation as an even more refined method of relative dating works through the comparison of similarities between defined units: for example, defined attributes or types. Three kinds of seriation are possible: phyletic, occurrence and frequency (O'Brien and Lyman 1999). Frequency seriation is the most common, its aim being to 'order the units in such fashion that the relative frequencies of artefact types within units follow a unimodal curve across units, so that each type monotonically increases and then decreases in frequency after reaching a peak of popularity' (Porčić 2013, 4552). If this is done, the resulting sequence is seen as a relative chronological one (Dunnell 1970). Over the last decades, correspondence analysis has been the preferred method of analysis. The normal outcome of a successful seriation is a 'battleship curve' of attributes or types, which is visually inspected (Fig. 3.8). It is worth noting some of the underlying logic, for example that the battleship curve is the predictable consequence of random cultural transmission or neutral evolution (Porčić 2013, 4552; Lipo *et al.* 1997; 2015). Further statistical tests and algorithms are being developed (Porčić 2013; Lipo *et al.* 2015). I have already noted Neolithic applications above; further good examples come from the ceramic sequence in the upper Rhine valley, both principally using decorative motifs (Lefranc 2007; Denaire 2009) (see also Fig. 4.26). Given the success of such studies, it is surprising that the approach has not been applied more across the range of Neolithic ceramic and other material studies.

So here are two tried and tested methods of establishing relative sequence, even though both are open to further development, and typology especially to more regularisation. Neither, however, in itself gives an absolute timescale, and that has conventionally been built up

Formal chronological modelling: the Bayesian approach to the interpretation of radiocarbon dates

Combined with a formal approach to the modelling of radiocarbon dates, however, all this potentially changes, and this book is written in the belief that the Bayesian approach to the interpretation of radiocarbon dates not only is currently the best available, but also provides us with powerful new insights into historical process in the European Neolithic, and of course far beyond. I write as a ‘Bayesian believer’, for reasons detailed below, but I stress that I am neither a radiocarbon physicist nor any kind of statistician. ‘Maths is something of a mystery to him’ laments my school report at about the age of fourteen, and I empathise with Winston Churchill in the wonderful book on his youth, *My early life*, when he recounts his puzzlement at the insistence of his governess on getting numbers right:

... figures were tied into all sorts of tangles and did things to one another which it was extremely difficult to forecast with complete accuracy. You had to say what they did each time they were tied up together, and the Governess apparently attached enormous importance to the answer being exact. It was not any use being ‘nearly right.’

(Churchill 1974, 11).

So I give a lay account of the Bayesian approach and the challenges which this throws up, while pointing the reader to a selection of the ever-increasing technical and specialised literature.

First, we need to go back to radiocarbon dating. Other things being equal, this works, and modern archaeology in general and what we have called prehistory in particular would be unrecognisable without it. I do not need to set out here the principles on which it works (Taylor and Bar-Yosef 2014; S. Manning 2015). We can reassure ourselves that radiocarbon dating does work, by reference to the measurement of samples of known age. I am not aware of any remaining fundamental doubters, though certain problematic results with the dating of bone, for example, whose chemistry is complex (Bayliss *et al.* 2011, 43–4; Bayliss *et al.* 2016, 34–5; Taylor and Bar-Yosef 2014, 75–82), seem to encourage some researchers (such as recently Gronenborn *et al.* 2014, 79; Strien 2017)⁶ to think that bone samples are potentially suspect in general, which is categorically not the case. But both laboratory physicists and archaeologists need to work hard to ensure that nothing goes wrong, and if it does, that appropriate corrections are made. There is an ongoing international programme of inter-laboratory comparison (Scott *et al.* 2010). Things can go wrong in lab process, such as the inadvertent inclusion of old carbon in the system used in AMS machines to filter the atoms to be measured (Bronk Ramsey *et al.* 2004). In our *Gathering Time* and *The*

Times of Their Lives projects, we have always submitted samples from the same site, and selected replicate samples (the very same sample material, split into two), to more than one laboratory; checks of this kind, much more immediate than through the formal interlab comparison, should speedily identify any problem, and lead to its correction (see also Bayliss *et al.* 2016, 46–50) (Fig. 3.9).

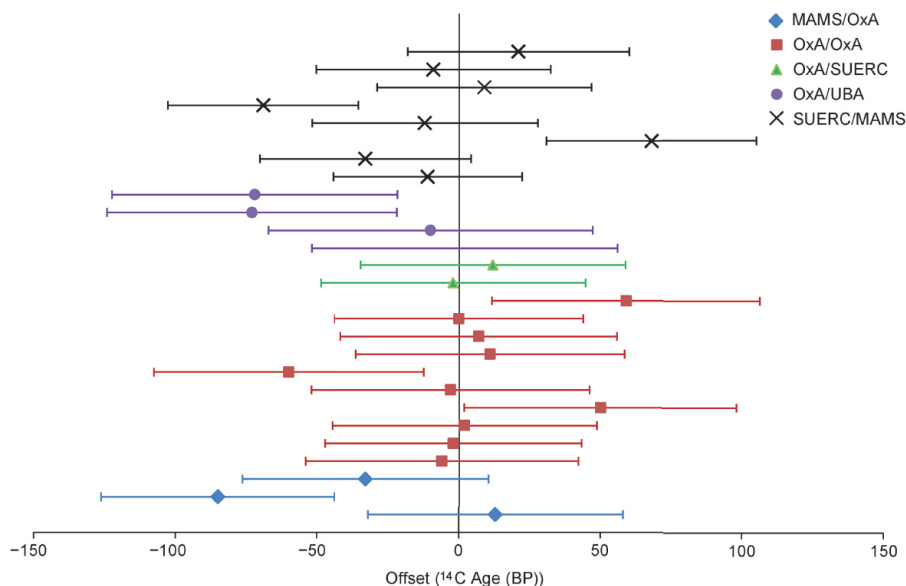


Fig. 3.9: Interlab variation: offsets between pairs of replicate radiocarbon measurements (error bars at 1 sigma) for Alsónyék (after Bayliss *et al.* 2016).

From the archaeological side, we have to work hard to ensure that the context and taphonomy (or biography, including after deposition) of samples are correctly identified, ideally so that samples to be dated can be said with full confidence to be of the same age as the context from which they were recovered. In our *Gathering Time* project, we operated with the mantra that ‘all samples are residual unless proven otherwise’. That requires a formal hierarchy of preferred samples. At the top of the list are samples from articulated and articulating (where all the anatomical elements are present in a given closed context, but not necessarily in their correct relative positions) human and animal bones, or quantities of charred plant remains or charcoal of short-lived species functionally associated with the context to be dated, such as a hearth or the floor of a house; at the bottom of the list are disarticulated animal bones, which could be significantly older than the contexts in which they are found, or bulk charcoal from long-lived species such as oak (Bayliss *et al.* 2011, 38–40; 2016, 37–8; see also

Killick 2015; Wood 2015). We have known this for a long time. Hans Waterbolk (1971) set out the principles getting on for 50 years ago! There are encouraging signs that the discipline is absorbing this, but it is still the case that less than ideal samples can be submitted, and some projects still create radiocarbon databases with all available results included, regardless of their quality or suitability.

We can note that non-ideal but properly characterised samples can still be used in the modelling process, generally as *termini post quos*. It is also now possible to exploit ‘charcoal outlier analysis’ in the models, to make a series of assumptions about the average age of a set of charcoal samples (Bronk Ramsey 2009). It is also necessary to check, with human bone samples, that there is no systematic age-offset (or difference in date) induced by diet, through the uptake of old carbon, generally from marine or calcareous freshwater sources. In our *The Times of Their Lives* project, we did this where possible by submitting ‘perfect pairs’, samples of human and terrestrial animal bone from the same closed contexts (Bayliss *et al.* 2016, 40–6).

There is now the further check of the FRUITS programme, where the isotopic values of samples can be used to model the possible constituents of the human diet represented by a given set of samples (Fernandes *et al.* 2014). Could fifth-millennium cal BC people living near the Danube in western Hungary, for example, have been eating significant quantities of fish, which could result in their appearing older than they really were? Our checks suggest that this was not so, though this is not to claim that this was never the case across Neolithic Europe. People living on or near the Baltic coasts, for example, or during the Mesolithic–Neolithic transition in the Danube Gorges, are relevant examples (Robson *et al.* 2015; Cook *et al.* 2001; Bonsall *et al.* 2015).

The need to calibrate radiocarbon dates was established decades ago (Renfrew 1973b; Bayliss 2009, 124–5). The detailed shape of the calibration curve is established by international agreement, and the current version is set out by Reimer *et al.* (2013). We should note criticism of some of the detail (Weninger *et al.* 2010; 2011; 2015); that is for the radiocarbon community to sort. In practical terms, the biggest challenge remains the series of plateaus on the curve, which can make precision in modelled date estimates difficult. These plateaus are prominent in the later sixth and later fourth millennia, with other episodes elsewhere on the curve (Fig. 3.10). This can make modelling the developed phases of the LBK difficult, for example. But refinement in the shape of the calibration curve means that the situation has improved considerably. It was previously considered that

the later fourth millennium plateau prevented any precision in date estimates for the early stages of Grooved Ware settlement in Orkney (Ashmore 1998, 142), whereas now, given enough samples and smaller error limits on measurements, what in the 1990s was a undifferentiated plateau in the calibration curve for the late fourth millennium cal BC has come into focus as a series of micro-wiggles (Reimer *et al.* 2013) which can be employed as the basis for much more constrained chronologies (Bayliss *et al.* 2017). In other cases, models produce bimodal results, indicating probable brief duration, but without the ability to choose which peak to settle on.

So what are we to do with calibrated radiocarbon dates? Up until recently, and sadly still in some quarters, the unthinking default procedure has been simply to look at the calibrated date ranges, at either one or two standard deviations, and through this informal visual inspection or eyeballing, to deduce an answer. Doing this with single dates is obviously dangerous, since we know statistically that one in 20 measurements will fall outside the true age of the sample (Taylor and Bar-Yosef 2014, 29). It is also dangerous with groups of dates, since we know that measurements will ‘scatter’ around a given phenomenon, some falling before and some after its true start and end (Steier and Rom 2000). Informal procedure varies. Many researchers have tended to pick out the earliest or latest parts of a given range, with the result that things will often, erroneously, appear to have begun earlier, lasted for longer, and ended later, than was the case in reality. This can be shown by exercises with simulations (Fig. 3.11), where the right answer is known (Bayliss *et al.* 2007, fig. 5; 2011, 59, note 2).

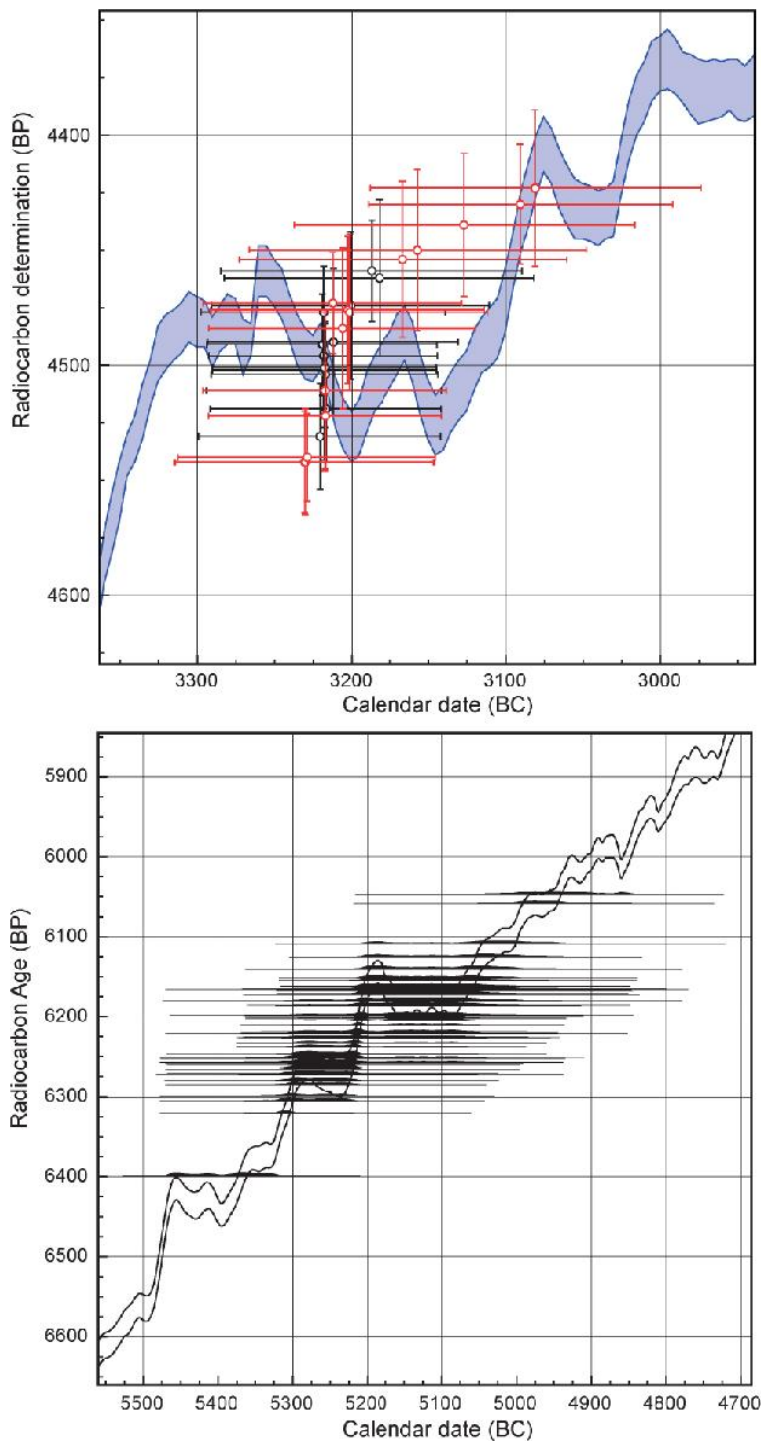


Fig. 3.10: Plateaus on the slope of the calibration curve. Top: later fourth millennium cal BC (after Chambon et al. 2018); below: later sixth millennium cal BC (after Jakucs et al. 2016).

Other researchers tend to pick, by entirely informal and never

explicitly recorded process, some kind of perceived average or median date, and promote date ranges prefixed by ‘around’ or ‘circa’. But even a single radiocarbon measurement with a reasonably small error can calibrate to a couple of centuries or more, and depending on the nature of the calibration curve at any one point in time, may offer a range of probabilities for the real date. And again, one cannot tell simply by looking at this range of probabilities what the correct answer may be. So it is the inescapable conclusion that, even if everything else is done well, in terms of both archaeological rigour in recording context and understanding the taphonomy of short-lived samples of the same age as the contexts they are chosen to date, and scientific reliability in the radiocarbon laboratories, something more has to be done with calibrated radiocarbon dates to get at their probable true age. Without this, it is no wonder that our default chronological perspectives have been those of the long term, and of generally slow change.

Two other formal methods have been tried, but both produce demonstrably imprecise results. The interquartile method, based on ‘frequentist’ or classical statistics, at least attempted formally to order groups of dates, drawing attention to the most populated, inner quartiles of the distribution of measurements for a given phenomenon (Ottaway 1973). This was done first with uncalibrated dates (cf. Geyh and Maret 1982; Breunig 1987) and later with calibrated dates (cf. Aitchison *et al.* 1991).

More recently, this has been developed further as ‘summing’, and large numbers of measurements can be conveniently and quickly processed through the OxCal and other programs. Some large-scale projects have simply gathered up all available measurements for a given phenomenon, in the belief that the signal will override any aberrant noise, and happily avoiding the hard work involved in assessing the reliability of each and every date (Shennan *et al.* 2013; K. Manning *et al.* 2014, 1065). But even with rigorously assessed measurements, summing is still inadequate, since it tends to reproduce the shape of the calibration curve and fails to offer sufficient precision (Bayliss *et al.* 2007, 9–11; Contreras and Meadows 2014). This can be seen especially clearly through the successive stages or phases of any sequence, when the tails of the summed distributions normally overlap. This is no escape from what we have called fuzzy chronology or fuzzy prehistory (Whittle *et al.* 2011, 914; Bayliss *et al.* 2016, 24).

On this basis, the Bayesian approach to the interpretation of radiocarbon dates is currently the best available. It is the basis on which this book rests, and it is being increasingly widely adopted,

though in turn that process brings challenges of rigour with it (Bayliss 2015; Buck and Meson 2015; Buck and Juarez 2017; Pettitt and Zilhão 2015). A good introduction to Bayesian statistics was given by the late Denis Lindley (1985), and a very detailed chronicle of the long story of the gradual acceptance –not yet universal – of the usefulness of the 18th-century theorem of Thomas Bayes (1763) is given by Sharon Bertsch McGrayne (2011). My favourite in her account is the exasperated response of President Johnson during the long search for a nuclear device which the American military had accidentally dropped in the mid-1960s in the waters off Spain: ‘I don’t want this probability stuff. I want a plan that tells me exactly when we’re going to find this bomb’ (McGrayne 2011, 193). But it was a Bayesian search method which did the job, and the warhead was located and recovered. Bayesian approaches are widespread in the modern world. Effective applications are cited in diverse fields such as biology, medicine, law, industry and computing (Pettitt and Zilhão 2015, 526), and also weapons testing and tracking, advertising and the internet itself (McGrayne 2011, 241–7).

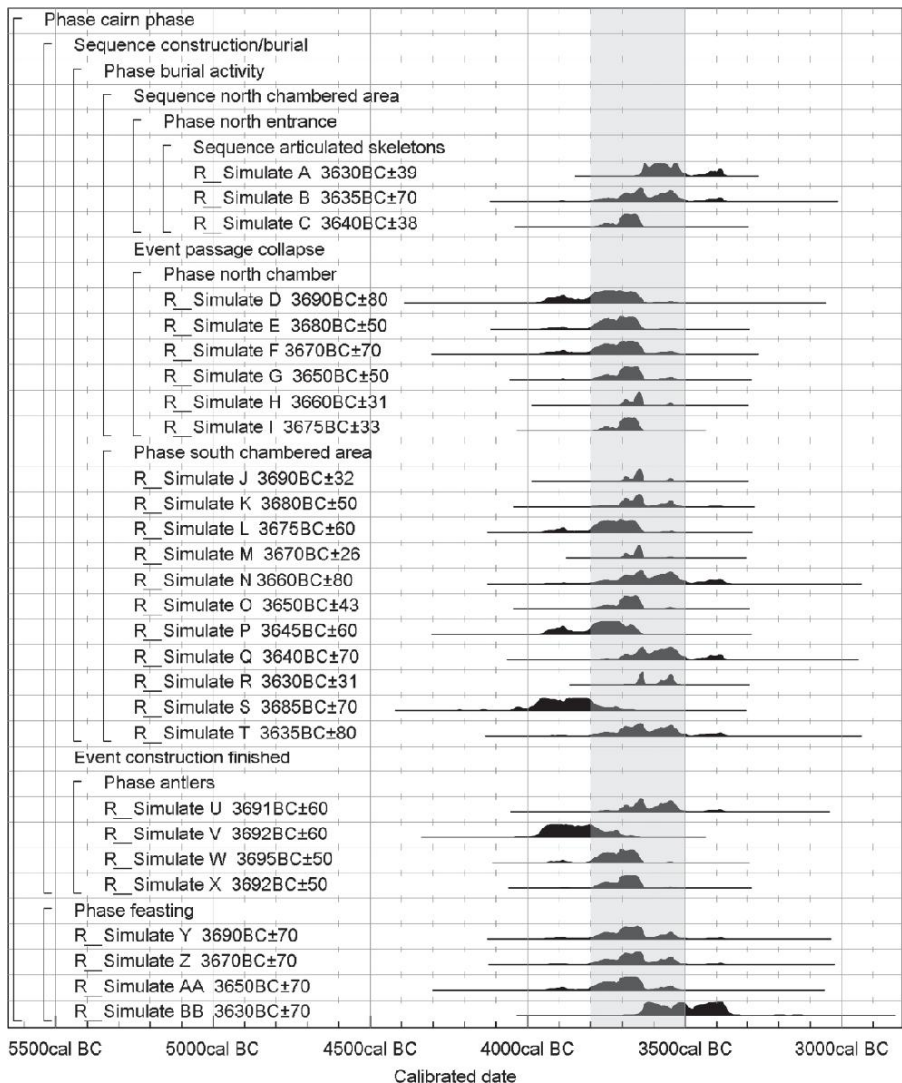


Fig. 3.11: *Simulation in action. Calibrated probability distributions of the simulated radiocarbon ages from a fictional long barrow, with a visual assessment of the most likely period when the site was in use (the actual dates of these samples range from 3695–3630 cal BC); after Bayliss et al. (2007).*

It is only fair to note the philosophical clash with classical or frequentist statistics. As Bradley Efron (2013a, 130) has put it, ‘By and large, Statistics is a prosperous and happy country, but it is not a completely peaceful one. Two contending philosophical parties, the Bayesians and the frequentists, have been vying for supremacy over the past two-and-a-half centuries’. He has noted two peaks in Bayesian popularity, two crashes and the current boom (Efron 2013b, 1177; chronicled in detail by McGrayne 2011). As he puts it, frequentism ignores past experience and concentrates on future behaviour; ‘an

optimal estimator is one that performs best in hypothetical repetitions of the current experiment. The resulting gain in scientific objectivity has carried the day, though at a price in the coherent integration of evidence from different sources' (Efron 2013b, 1178). And beyond the philosophical clashes here, there is the practical problem that there is currently no coherent or rigorous frequentist approach, using maximum likelihood estimation for example (Efron 2013a, 136), available for the interpretation of radiocarbon dates.

In contrast, the Bayesian approach explicitly takes account of past experience (Fig. 3.12). In the 18th century there was intense interest in probability, in the terms of the nonconformist minister Thomas Bayes a 'doctrine of chances', stemming from diverse concerns, including the outcome of gambling (McGrayne 2011, 6). Bayes' rule or theorem (1763) was submitted in an essay posthumously by his friend Richard Price. As Bradley Efron (2013a, 132) acidly comments, 'Price thought the rule was a proof of the existence of God, an attitude not entirely absent from the current Bayesian literature'. The theorem combines prior knowledge or assumptions with new information to come up with a new assessment of probability. As Scott Ortman (2016, 104) has put it, 'the theorem states that the probability (P) of a hypothesis (H), given a specific piece of data (D), is proportional to the probability of the hypothesis before the data are known times the probability of the data if the hypothesis were true'. While there is justifiable concern over what may constitute genuine prior information (Efron 2013b, 1178; 2013a), there is an abundance of relevant archaeological information which can be used, including detailed knowledge of context, associations, stratigraphy and the taphonomy of samples to be dated; as already noted, and as will be discussed in the next chapter, this can embrace typological schemes and seriations, thus incorporating what might otherwise be regarded as traditional or conventional means of ordering data into relative sequence.

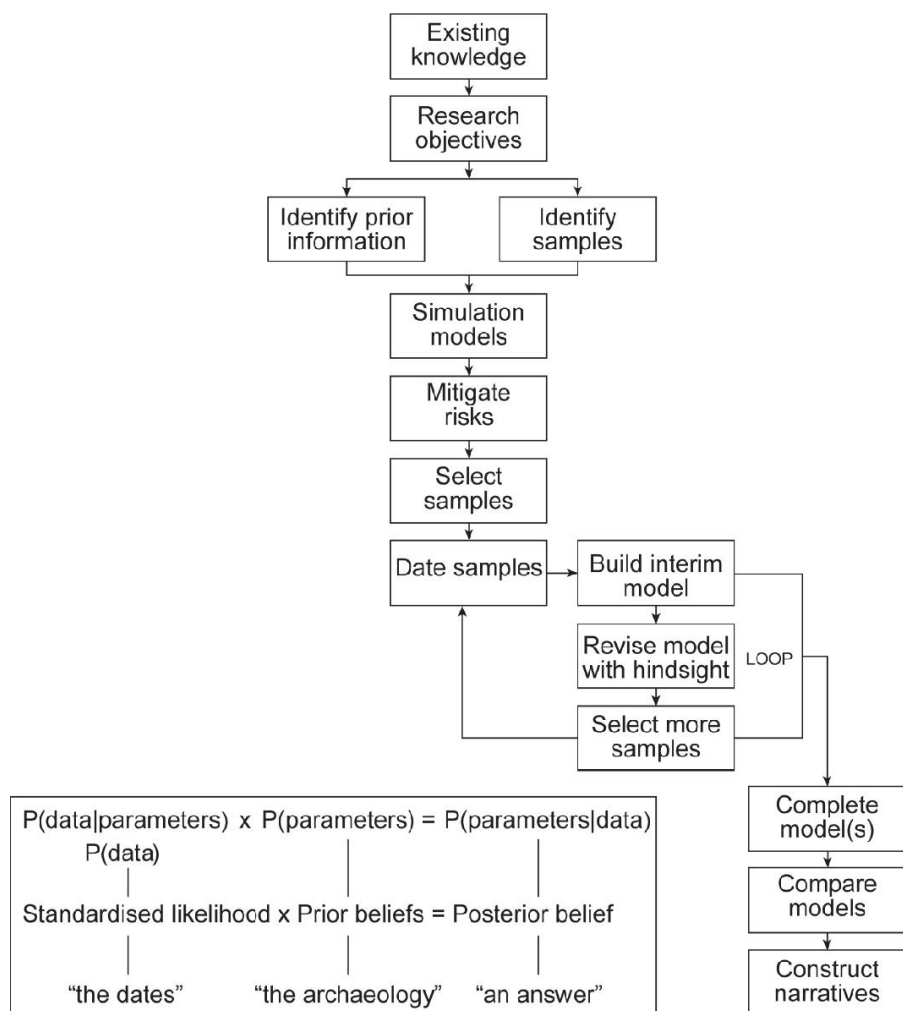


Fig. 3.12: The theorem of Thomas Bayes and the Bayesian process for formal chronological modelling. After Bayliss et al. (2016).

All these sources of information can go into the models as what are called informative priors, because they strongly affect the output of the models (Bayliss *et al.* 2007, 13); so-called uninformative priors, which have a more neutral effect on the output of models, are noted below. We have referred to the cable metaphor of Alison Wylie (2002); individual strands of evidence may be of relative strength, or weakness, on their own, whereas in combination they can constitute a very robust cable (Bayliss and Whittle 2015; Bayliss *et al.* 2016, 26). The Bayesian approach harnesses multiple sources of information, in powerful combinations.

The details of the application of Bayes' theorem to the interpretation of radiocarbon dates have been set out many times, and

I simply refer the reader to some of the best of those (Buck *et al.* 1992; 1996; Bayliss and Bronk Ramsey 2004; Bayliss 2009; in prep.; Bayliss *et al.* 2007; 2011; 2013; 2016). Following the first applications in the 1980s and 1990s (including Bayliss *et al.* 1997 on Stonehenge), there are clear signs in the literature now of an accelerating uptake of the Bayesian approach. But this welcome development should be accompanied by a note of caution. For effective outcomes, care and rigour are required at every step of the procedure. Alex Bayliss (2015; see also Buck and Meson 2015; Buck and Juarez 2017; Pettitt and Zilhão 2015) has observed, however, that roughly half of published applications, the great majority of which have appeared in the last few years, do not meet best practice in terms of sample selection or explicit reporting of model construction, in a way that can allow models to be critically assessed or alternatives reconstructed. This may be the third radiocarbon revolution (Bayliss 2009, 126–7), but as so often with revolutions the aftermath risks being messy.

One major practical challenge for the discipline at the moment is to generate enough trained modellers, who are still in short supply. I have had a privileged ringside seat over the last 15 years or more, but make no pretence to expert knowledge of model construction. What I can usefully pass on perhaps is experience of the need for rigour at all stages, and a sense of the inbuilt checks in the procedure of model construction, which should give the discipline confidence in the outcomes. A respected colleague, who shall remain nameless, commented during the writing of one of our ToTL papers, that the trouble with the Bayesian approach, in their view, is that too much power falls into the hands of modellers. Not so! This needs to be a cooperative effort. Archaeologists must bring critical knowledge of samples, and their context and taphonomy, while models must be described explicitly and fully in terms of both their informative and uninformative priors. In many of the earlier applications, one of the key uninformative priors is the assumption that the activity represented by a group of related radiocarbon dates can be modelled as a bounded but uniform phase – in plain English, as a phenomenon with a beginning, a middle and an end – with activity within that phase falling at any point within it. More recently, it has been possible to weight the possible distribution of activity within a given phase (Lee and Bronk Ramsey 2012), and it may be possible in future to envisage more open-ended starts and ends to phases, allowing more complex modelling of situations of potential overlap between phenomena (in real terms, such as the introduction of a new style of monument, say, or a novel pot style, while older ways of doing things

linger on for a while).

Properly presented models have inbuilt checks, in terms of measures of convergence (how quickly the millions of computations carried out by the computer take to settle on a preferred outcome) and agreement or internal consistency. These will often throw up problems during model construction, causing the team involved to rethink their assumptions or to identify outliers and misfits (Bayliss *et al.* 2016, 56–7). We can further reassure ourselves, and others, by reference both to repeated simulation exercises, where the real date of a hypothetical situation is known by back-calibration of radiocarbon dates and in which Bayesian modelling generates the correct answer (Bayliss *et al.* 2007, fig. 5; 2016, 38–40), and to the application of Bayesian modelling to floating tree-ring sequences, through so-called ‘wiggle-matching’ (English Heritage 1998, 19–20), which also regularly gives the right answer.

Above all, this is an explicit, quantified and probabilistic approach. It demands interpretation from all involved in model construction and subsequent commentary, as we move from chronicle to narrative. Explicitly, it has been emphasised that ‘absolute Bayesian rigour is almost impossible to achieve’ (Bayliss and Bronk Ramsey 2004, 35), and that

at present the validation of Bayesian models is an inexact science, although several statistical approaches have been developed which can assist in the identification of incorrect models and incompatible prior beliefs and standardised likelihoods [the new information provided by radiocarbon dates]. Statistics alone cannot be relied upon to identify all the incorrect components of a model, and archaeological critique of the character and context of the dated material and scientific understanding of the complexities of radiocarbon dating are key elements in model validation (Bayliss *et al.* 2016, 55).

Alternative models (‘sensitivity analyses’ in the technical language: Bayliss *et al.* 2007; 2016) can and often should be constructed; the Bayesian process should be an iterative one. Sometimes the outcomes are rather similar, which can be reassuring; on other occasions, they may be rather different, which requires judgment and reasoned choice of the preferred model, at least for the time being. ‘All models are wrong, some models are useful’, declared the British statistician, George Box (1979, 202), to which he might have added, ‘for a while’. Fresh excavations can take place or archives be reassessed, new typologies or seriations can be created, more and better samples can become available, and refined modelling should be developed.

All this should allay any fear that the Bayesian approach is somehow too good to be true. It is far from perfect. The approach generates explicit and quantified models; alternative models can and

should be constructed, and fresh information allows them to be recast. Reasoned interpretation is required, and it is perhaps this unfamiliar combination of hard numbers and archaeological judgment, apart from a suspicion of the black box of the computer (with technicalities such as random Monte Carlo Markov Chain sampling, the Metropolis-Hastings algorithm and the Gibbs sampler: Bayliss *et al.* 2007, 6) and a general fear of statistics (having to enter what Churchill (1974, 12) labelled ‘a dismal bog called “sums”’), which is most unsettling to newcomers. But the Bayesian approach, open though it is to future refinement, is the best we have got at the present time. Without it, we are extremely likely to get the wrong answers. With it, we can powerfully pull together disparate sources of information, and generate a much more precise sense of timing, duration and tempo, routinely when things go well down to the timescales of lifetimes and generations. I go on in the next chapter to show how this is so, across a wide range of applications to Neolithic Europe.

Notes

1 Tim Ingold (2007, 90–1) also discusses plot, as part of a more circuitous storyline, related to his notion of wayfaring.

2 By convention, dendrochronological dates are given as BC, while calibrated radiocarbon dates are given as cal BC (Mook 1986).

3 Subject to the reservations noted above.

4 It is worth noting that the conditions of preservation encountered in the more recent excavations were considerably worse than those found in the original investigations in 1944.

5 There are plenty of qualifications to be made. For example, the seriations of Höhn (2002) showed more overlap between phases than Lüning has originally proposed; the *Ösenkranzflaschen* and *Ösenleistenflaschen* used by Lüning to define MK III and MK IV respectively have been shown to overlap in their use (Matuschik 2011); and debate continues about the separate existence and geographical focus of MK I. I am grateful to Ute Seidel for her guidance here; and see Seidel 2017a.

6 This is in the context of ongoing debate about the date of the start of the LBK in central Europe. Hans-Christoph Strien (2017) discusses three ToTL papers, in which many but not all samples are of short-lived animal bone: Tasić *et al.* 2016a; Jakucs *et al.* 2016; and Denaire *et al.* 2017. He appears to accept the suggested sequences and chronologies for Vinča-Belo Brdo and the long sequence in Lower Alsace, though not explicitly acknowledging the rigour of ToTL sample selection or the robustness of a formal approach in general. Selectively, he picks out date estimates for Szederkény in particular and the earliest LBK in general as being too young (that is, later than supported by his own informal estimates) and suggests some kind of diagenesis in fluvial conditions as the culprit (cf. Gronenborn *et al.* 2014, 79). Partly this is alleged on the basis of very simple comparison with cereal remain samples, and partly this is argued on the basis of a perception of the over-fast spread of the LBK. Demographic implications aside, the difference in outcomes could be checked by a systematic comparison of samples of different materials in a series of contexts (see for example Bayliss *et al.* 2011, figs 2.31 and 2.32); as things stand, it seems inconsistent to accept some formal date estimates, while rejecting others simply when they diverge from preferred informal estimates.

Chapter 4

‘Sometimes sudden and sometimes slow’: the reach of precision

In the wonderful novel by Carson McCullers, *The member of the wedding*, the young heroine Frankie discusses life with the family maid, Berenice.

‘The world is certainly a small place’, she said.

‘What makes you say that?’

‘I mean sudden,’ said Frankie. ‘The world is certainly a sudden place.’

‘Well, I don’t know,’ said Berenice. ‘Sometimes sudden and sometimes slow.’

This conversation neatly catches a lot that we could say about the pace and nature of change in the Neolithic past. The challenge is how we can hope to discriminate between the sudden and the slow. Previous approaches have tended to lump phenomena of potentially disparate character together, smearing the chronology, such that only the biggest breaks stand out from a default picture of continuity and gradual change. In the last chapter, I set out how a Bayesian approach to the interpretation of radiocarbon dates, importantly in combination with other information, is currently the best available method for achieving precise date estimates, apart from those restricted areas blessed with dendrochronologies. In this chapter, I present a series of Neolithic and Copper Age case studies of this approach in action, looking at settlement, monuments and material culture. I want to bring out the often much more dynamic and punctuated sequences that result, compared to what we have previously constructed. The pace and character of change are not, however, everywhere the same, and I pursue the further implications of that claim in the two chapters that follow.

Living well together?

One of the great strengths of Neolithic archaeology across Europe is the wealth of studies of settlement, built up by over a century of

research. In this section, I am going to look at settlement patterns, the lives of different kinds of residential site, and the biographies of houses. Getting to grips with these themes is not just a task of description of residential arrangements, but also centrally involves the sociality and values of community (Overing 2003; Overing and Passes 2000; Bailey *et al.* 2008).

Life on the ground

There have been studies of settlement in practically every region of Europe (Whittle 1996; Fowler *et al.* 2015). There are obviously many big and visible contrasts, in the characteristic forms of settlement, their distribution across the landscape, their varying density and finally the major changes that take place through the millennia of the Neolithic sequence. The world of tells and large ‘flat’ sites in south-east Europe down to the middle and end of the fifth millennium cal BC appears very different to that of the later sixth millennium cal BC LBK longhouse settlements in central and western Europe (and not forgetting the eastern LBK distribution). Both in turn contrast with the seemingly more dispersed and certainly much more elusive traces of settlement characteristic of large parts of both south-eastern and western Europe from the later fifth millennium cal BC onwards. The richly documented settlements of the fourth and earlier third millennium BC in the Alpine foreland are another, puzzling, point of difference in an age of much reduced visibility of residence elsewhere in western Europe. Unsurprisingly, in most areas people chose, over and over again, what appear to be the best locations in the landscape for the operation of the new agricultural economy, on the best soils and near water in the river valleys. There are of course exceptions to this big generalisation, but it is normally only later and gradually that traces of settlement away from the valleys and into the uplands are found. To take just two examples, the studies of southern Poland (Kruk 1980) and the Great Hungarian Plain (Sherratt 1982) remain classics: the former documenting a long-term shift from valley-centred residence in the LBK and Lengyel periods, to the edge of the interfluvies in the TRB period; and the latter following a shift from dispersal to nucleation and back to dispersal, by the second half of the fifth millennium cal BC, and tracking links with the surrounding uplands. In occupied parts of the landscape, the density of settlements can be high – witness the numbers of tells on the Thessalian plain in northern Greece (Andreou *et al.* 1996), or ribbon-like LBK settlement along rivers and streams across the loess soils (Zimmermann *et al.* 2009) of central Europe – assuming that all the sites assigned to conventional phases were directly contemporary. In southern Iberia,

to jump southwards, it is harder to follow the development of settlement in comparable detail, but it certainly appears that there were more sites from the later fourth millennium cal BC onwards, including a series of impressive larger settlements (R. Chapman 2008; García Sanjuán and Murillo-Barroso 2013).

Clearly this was not a static world, and things changed over the long run. But it is striking how often studies have been presented within very broad chronologies – far removed from the precision detailed for the Alpine foreland in the previous chapter. So the currency of the southern Polish, Hungarian and southern Iberian studies, for example, has consistently been in blocks of centuries. The narrative of life on the ground, from generation to generation and from century to century, becomes unavoidably one of continuity on the one hand and slow change on the other, with only major breaks standing out as potential disjunctures, and with even those mostly rather poorly timed. One otherwise meticulous and authoritative study which covered the end of tell settlements in south-eastern Europe deliberately avoided the use of radiocarbon dating altogether, though it did importantly identify, using just material culture and especially pottery, a process of abandonment beginning earlier in the northern compared to the southern part of the Balkans (Parzinger 1993). Subsequent studies, using a mixture of informal visual inspection of radiocarbon dates and limited formal modelling, have identified the middle of the fifth millennium cal BC as the horizon of major change in the northern part of the Balkans, with tell endings possibly spread across the 47th and 46th centuries cal BC, but with little comparable relative precision available for the succeeding period of dispersed settlement (Link 2006; Borić 2009; 2015).

The ending of ‘Danubian’¹ or LBK/post-LBK traditions in central and western Europe is another illuminating example. This is the point when several centuries, if not almost a millennium, of the use of longhouses in their valley settings came to an end, and the rather different world of dispersed settlements and more visible monuments emerged. But when was this, and was there a single date which applies across the very broad area in question? Most researchers would probably identify in broad terms a horizon in the middle to later part of the fifth millennium cal BC, but that is imprecise and unambitious, from both local or regional and wider perspectives. On a regional basis, there seems to be wide agreement that the Michelsberg culture, which is emblematic of the post-Danubian world, probably emerged to the west, in northern France, and spread eastwards, but there is a healthy debate about whether a high or a low chronology –

at around 4500 cal BC or around say 4300 cal BC – better applies (Jeunesse *et al.* 2003; Denaire *et al.* 2017). This is so far unresolved, and this matters, not least from a wider perspective and with the possibility in mind that the big systems of tell settlement in parts of south-eastern Europe and longhouse settlement in central and western Europe could both have come to an end at about the same time. And if that were the case, what would be the implications?

There is one exception to the default broad-brush chronology normally employed in such settlement studies. I have already cited in the previous chapter the many detailed typological studies of decorated LBK pottery, with further correspondence analysis and seriation in some cases. This has generated many sharp, focused regional studies. To take just one example, this leads to detailed observation of the shifting trajectory and intensity of LBK settlement in the Wetterau, part of the drainage of the Main, in turn tributary of the middle Rhine (Ebersbach and Schade 2005). Combined especially with the *Hofplatzmodell* (Zimmermann 2012), by which LBK longhouse settlements are broken down into successive series of independent longhouses or farmsteads, each within its own space, and being replaced at a regular pulse, normally estimated as fewer than 30 years, this has produced an even finergrained, generational chronology in areas with substantial excavation. The classic example has long been that of the Aldenhoven plateau in north-west Germany, where the vast opencast lignite extraction works led to extensive excavation of ribbon-like LBK settlement along the local stream valley, the Merzbach, from the 1960s onwards (Lüning and Stehli 1994); continued rescue and research have documented further LBK settlement in adjacent stream and small river valleys (Classen 2009 and references). Up to 14 phases were identified in the Merzbach, beginning with pioneer occupation of Langweiler 8, which went on to become the locally largest longhouse settlement (Lüning and Stehli 1994).

Another comparable study, of the large, nucleated LBK longhouse settlement of Vaihingen, in a tributary of the Neckar valley in south-west Germany, has identified up to 17 house generations. The ceramic study in this instance identified five groupings of adjacent houses, each with distinctive sets of decorative motifs; these clusters were proposed as lineage groups or ‘clans’. In turn, these groupings correlate closely with fine differences in the composition of crop and weed assemblages, which cutting a long story short has been taken to signify varying access to the local soils (Strien 2005; Bogaard *et al.* 2011). Intriguingly, the ceramic groupings or clans within the

Vaihingen nucleation can be related convincingly to two major regional styles in the wider LBK social landscape. Thus, groups or clans A and D are associated with the local Middle Neckar tradition, to the south of Vaihingen, and B, C and E with the Unterland-Kraichgau style to the north (Bogaard *et al.* 2011, fig. 5). And there is a further dynamic history, because the house groupings appear to have been of unequal duration. While the settlement as a whole is given a long life, of around 450 years (Bogaard *et al.* 2011, 399), the site reduced in size at the local ‘Flomborn to middle LBK transition’, estimated at a little before 5200 cal BC (Bogaard *et al.* 2011, 402, and fig. 3), with the ending of groups B, C and E; it is possible that house group A had privileged access to the closest and perhaps best local plots, and the undoing of group C at least might be related to its less favourable access (Bogaard *et al.* 2011, 408, and fig. 7).

So what I have called conventional approaches, though in these instances clearly using a range of methodologies, can help in the construction of dynamic settlement histories. It is important to point out, however, a so far unresolved disparity in the estimates for the overall span of the LBK, and also for the durations of individual longhouses. I will come back to both below, noting here especially that the local estimate for the start of the earliest (*älteste*) LBK in the Middle Neckar just after 5500 cal BC (Bogaard *et al.* 2011, 399, and fig. 3) is at odds with formally modelled estimates for the start of the earliest LBK over a much wider area, probably in the 54th century cal BC (Jakucs *et al.* 2016). It seems obvious that a major radiocarbon dating and modelling programme for Vaihingen, exploiting the wealth of human, animal and plant remains, would be the next step in resolving this chronological clash. We can also note that the end of Vaihingen as a whole is estimated as falling in the earlier part of the 51st century cal BC (Bogaard *et al.* 2011, 399, and fig. 3).

The comings and goings of long-term settlement history: Lower Alsace

One of our most thought-provoking case studies in the ToTL project involved formally modelling the long Early and Middle Neolithic sequence in Lower Alsace, in eastern France (Denaire *et al.* 2017), as already noted in [Chapter 1](#). The area we looked at is on the west side of the upper Rhine, only some 55 km from south to north, and some 25 km from river to flanking uplands of the Vosges. There is also a narrow strip on the east side of the Rhine (in Germany), before the uplands of the Black Forest ([Fig. 4.1](#)). Much of the rich Neolithic archaeology of the region comes from rescue and contract excavations on the loess-covered terraces of the valley, though an older tradition

of research investigations can be tracked back to the late 19th and beginning of the 20th century (Lefranc 2007; Denaire 2009). The Early Neolithic sequence begins in the latter part of the sixth millennium cal BC, with the appearance and development of the LBK (*Rubané* in French). The Middle Neolithic sequence covers much of the fifth é millennium cal BC, from the Hinkelstein, Grossgartach, Planig-Friedberg and Rössen phases, on to the Bischheim, Bruebach-Oberbergen, and BORS (*Bischheim Occidental du Rhin Supérieur*) phases. From the end of the fifth millennium cal BC, a rather different scene emerged, with a series of contrasts to this initial ‘Danubian’ world, and dominated by the appearance of the Michelsberg culture.

Previously, the absolute dating of these successive cultural phases in Lower Alsace and, more widely, across the upper Rhine valley as a whole, rested on a small number of radiocarbon dates. Moreover, a number of these phases, in particular those from the end of fifth millennium, were only indirectly fixed in time, through comparison with the better dated sequences in neighbouring regions such as the Alpine foreland. The arrival of the LBK in the Rhineland was previously estimated at about 5300 cal BC and its end placed a little after 5000 cal BC, based on a dendrochronological date from Erkelenz-Kückhoven much further down the Rhine (Lefranc 2007; Denaire 2009; 2011). The beginning of the Hinkelstein phase (and the Middle Neolithic) was thought to overlap with the end of the LBK, somewhere around 4950 cal BC, and from those developed the Grossgartach phase. This culture, with the Planig-Friedberg group, was seen to persist until after 4750 cal BC, and to be replaced by the Rössen culture, itself succeeded about 4450 cal BC at the latest by the Bischheim group. That in turn evolved locally into the Bruebach-Oberbergen group about 4300 cal BC, which was supplanted by the BORS group a little after 4200 BC cal BC. The arrival of the Michelsberg culture, about 4000 cal BC, marked the end of the BORS and the Middle Neolithic sequence in Lower Alsace.



Fig. 4.1: Fifth-millennium archaeology in Lower Alsace. Clockwise from top left: a Grossgartach house at Bernolsheim-Mommenheim PDA de Brumath (photo: Archéologie-Alsace); Planig-Friedberg grave at Obernai (photo: Philippe Lefranc); Grossgartach pottery from Oberschaeffolsheim (photo: Anthony Denaire); Grossgartach pits and other subsoil features at Kolbsheim-Vogeseblick (photo: Anthony Denaire).

We modelled just over 150 radiocarbon dates for this sequence, including checking carefully against the possibility of dietary offsets. Samples came from a series of closed assemblages in graves and settlement pits. Samples were chosen to date phases indicated by the seriations available for the decorative motifs on pottery throughout most of this long sequence (Lefranc 2007; Denaire 2009; cf. Bayliss *et al.* 2013): a good illustration of the way in which the Bayesian approach combines detailed archaeological analysis with radiocarbon dating. Without going into all the detail through the whole sequence (set out in Denaire *et al.* 2017) and by way of example, the seriation of

LBK ceramics used 112 assemblages from 20 settlements and was based on 87 typological criteria, including rim motifs, the types of bands which formed the principal decorative structure, and the secondary motifs which occur in the spaces in between (Lefranc 2007). The correspondence analysis was partitioned into seven phases, which we see as showing temporal sequence. The first phase includes assemblages equivalent to the Flomborn style of the earlier LBK in central Europe, and the last, that of the Dachstein style, traditionally identified as the final LBK style in Lower Alsace. The succession of chronological phases defined by the seriation is confirmed by numerous stratigraphic relationships in the settlements of the region. For purposes of comparability, we kept the structure of the chronological scheme of Meier-Arendt (1966), used since the first systematic periodisations of the LBK in Alsace. In that scheme, the LBK was divided into five main stages: the earliest LBK I (*älteste* LBK); an early LBK II; a middle LBK III; a late LBK IV; and a final LBK V. All but the first of these typological stages can be seen in the correspondence analysis for Lower Alsace, which enables the subdivision of LBK II into two, phases LBK IIb and IIc, and LBK IV into three phases, LBK IVa1, IVa2 and IVb. It has not proved possible to subdivide the middle (LBK III) and final stages (LBK V). In this way, we were harnessing existing expert understanding of the material.

What came out of the modelling was a high-precision, generational chronicle of the ever-changing nature of the pottery in the study area (Fig. 4.2). There were some obstacles and some surprises. Despite exhaustive search, we could not find samples to date LBK V, and a five-fold partitioning in the Grossgartach seriation proved not to be temporal. I am going to come back to this material in its own right later in the chapter, but here I want to use the timing and tempo of the sequence to build a narrative of the repeated shifts of life on the ground. I will deliberately stick quite closely to our original account (Denaire *et al.* 2017), in order to convey a full sense of the detail.

In our model, the appearance of the LBK in Lower Alsace in LBK IIb was rapid. In the space of two generations, nearly all the loess areas were densely occupied. Only the north part of the Kochersberg plateau, north of Strasbourg, and the Andlau terrace, in the south of Lower Alsace, were unoccupied. With 36 sites known, LBK IIb marks a surge in the settlement of the region, which is probably best explained by the influx of a new population. The origin of these people was probably in the Neckar region or in the *Rubané du Nord Ouest* region, which in its early phase shares numerous traits with Lower Alsace (Lefranc 2007). The transitional LBK IIc phase is also short-lived, and

things run on into phase III. In LBK III, the number of known sites falls to 17. This apparent decrease translates to the abandonment of numerous sites and the absence of newly founded settlements. This phenomenon, observed in other regions (and note again the history of Vaihingen, detailed above), is sometimes associated with a new period of mobility and expansion to the west (Strien and Gronenborn 2005).

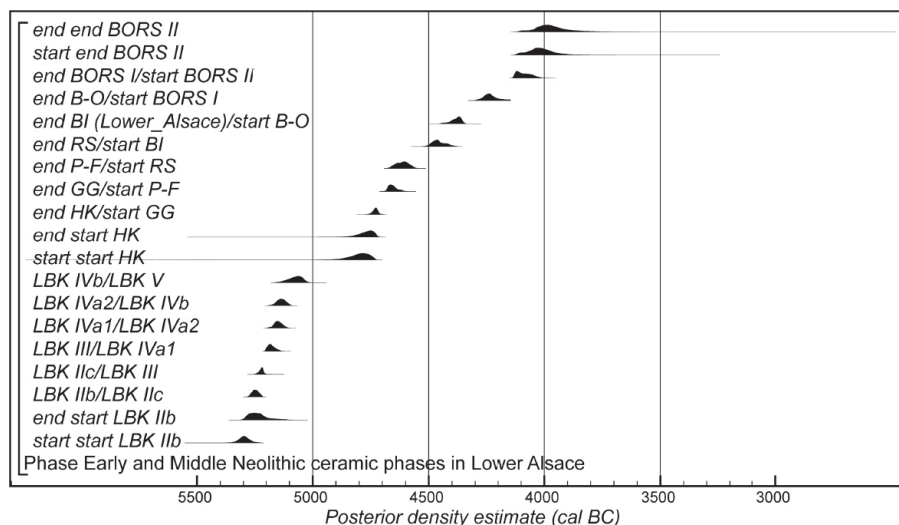


Fig. 4.2: Model for the LBK (Rubané) and Middle Neolithic ceramic sequence in Lower Alsace. After Denaire et al. (2017).

In LBK IV, more than 70 sites are known in Lower Alsace. In the first half of this stage, settlement stays relatively stable. The pattern of settlements continues from phase III. The relatively long phase IVb (two or three generations) is then marked by an increased number of settlements, which double compared to phases IVa1 and IVa2 (which together persist for a comparable two to three generations); these include now seven new settlements to the north of Kochersberg, in a new area previously uncolonised. Phase IV in Lower Alsace is also marked by the appearance of large cemeteries, such as Vendenheim (Boës *et al.* 2007), Souffelweyersheim (Jeunesse 1997) or Ingenheim (Lefranc *et al.* 2014). In contrast to what is known in the neighbouring regions of the Rhine and Upper Alsace, no cemetery is yet attributable to LBK II and III in Lower Alsace (Lefranc *et al.* 2014; Denaire and Lefranc 2014). The dead now became a socially significant resource and point of reference. Distinct funerary rituals at such sites could be the expression of different local identities (Boës *et al.* 2007; Jeunesse 2009). In the context of settlement intensification, it is worth noting the suggestion that, on the basis of similarities in the pottery, the LBK

in the Aisne valley could be derived from Lower Alsace, by a process of outward migration, via the Marne (Jeunesse 2008). After the dynamic shifts which characterise phase IVb in Lower Alsace, there was a dramatic shrinkage of the LBK occupation of Lower Alsace in phase V. The Kochersberg plateau, densely populated in phase IVb, becomes deserted and LBK occupation is concentrated in a small area along the foothills of the Vosges uplands.

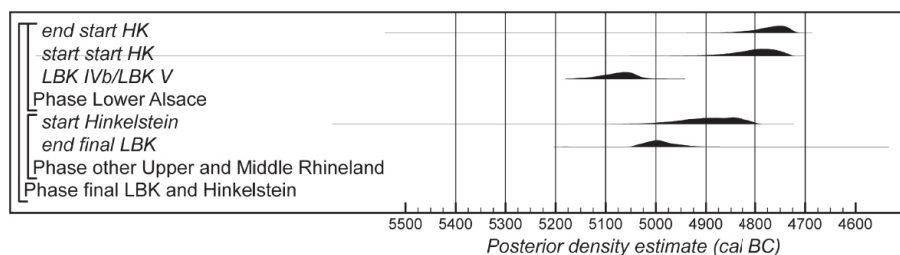


Fig. 4.3: Model for the end of use of LBK pottery and the start of use of Hinkelstein ceramics in Lower Alsace and the middle and upper Rhineland. After Denaire *et al.* (2017).

An even bigger surprise came at the end of the LBK and the transition to the Hinkelstein phase, previously believed to overlap. Our models indicate that there was probably in reality a hiatus in Lower Alsace between the final LBK occupation towards the end of the 51st century cal BC and the appearance of the first Hinkelstein pottery in the first half of the 48th century cal BC! To try to assess this result and to put it into context, we gathered up a rather miserable collection of other existing dates (another obvious target for future research) from elsewhere in the Rhineland. Cutting another long story short (set out in detail in Denaire *et al.* 2017), it appears that the last LBK pottery in the Rhineland was used in the last decades of the 51st century cal BC in a number of areas, including in the Neckar Valley at the well-known mass burial pit at Talheim; note again the estimate for the end of Vaihingen given above. In other regions of the Rhineland for which we currently have radiocarbon dates, there also appears to be a hiatus between the end of the LBK and the appearance of Hinkelstein ceramics, probably in the order of 40–175 years (Denaire *et al.* 2017, fig. 24), or some two to seven generations (Fig. 4.3).

This challenges conventional understanding, for example of supposed contact finds in the core Hinkelstein area around Worms in the middle Rhine valley (Meier-Arendt 1975, 128–31). But the likelihood of a gap is also suggested by the lack of Hinkelstein sites in Upper Alsace. A similar situation exists in the region of the Aldenhovener Platte, where the sequence appears to resume with

Grossgartach sites. In this case, regional pollen diagrams, with regeneration followed by indications of renewed clearance (Kalis and Meurers-Balke 1988), appear to provide supporting evidence for a break or decline in settlement. A compromise may be to envisage a short gap in some areas, as around Worms (and somehow dating the earliest Hinkelstein graves in that area is another obvious target for future research), but a longer hiatus elsewhere. Once again, a much more nuanced picture emerges after formal modelling. There are obviously many further implications stemming from this outcome. We are simply not used to thinking about major discontinuity. One recent account, linked to discussion of the effect of climatic volatility, proposed that 'LBK reaches its highest population rates and at the same time experiences a period of warfare. Thereafter population rates decline and LBK *gradually* vanishes from the archaeological record, being replaced by Middle Neolithic societies' (Gronenborn *et al.* 2014, 73; my emphasis). We considered the possibility of other factors being involved, including disease (Denaire *et al.* 2017).

When occupation was finally resumed, the Hinkelstein style now appears to come into Lower Alsace rapidly, in less than 20 years. A short duration of less than two generations may be part of the explanation for only five sites being known. These are found around the edges of the areas formerly occupied by the LBK, quite close to the Rhine, which was presumably the communication route with the middle Rhine, from where the Hinkelstein style is thought to derive. It is only during the Grossgartach phase that all the territory occupied during the LBK was re-inhabited (Denaire 2009, 255–66). With a duration of perhaps only three generations, the density of Grossgartach sites in Lower Alsace is striking, matching that of the late LBK, though sites were smaller; hamlets of two or three contemporary farmsteads may be an appropriate model (Denaire 2014). So the recolonisation of Lower Alsace in the Hinkelstein and Grossgartach phases was not the same as at the start of the LBK sequence. It was more limited, and the first steps involved few sites; it took two or three generations to take in the whole of the area previously occupied in the LBK.

Thereafter things seem to settle down. The Planig-Friedberg style was a relatively short transitional phase (Denaire 2009; Denaire and Mauvilly 2012), of about a generation or so, between Grossgartach and Rössen. From large-scale excavations of settlements and cemeteries (see again [Fig. 1.4](#)), we can see that sites started in the Grossgartach phase continue through Planig-Friedberg to the Rössen phase. The Rössen phase, over four to five generations, lasted longer

than Grossgartach, but is less visible in Lower Alsace, given the 30 sites known, because of changes in the nature of features. Big pits, so common in the Grossgartach, were no longer dug, and the small pits of settlements are harder to detect and although some cemeteries remain in use, no substantial new one was started in the Rössen phase. In the Lower Alsace and wider Rhineland sequence, this is where we start to lose sight of the Danubian longhouse (see further in [Chapter 6](#)). There was, however, an expansion of the areas inhabited, for example north of the forest of Haguenau (Denaire 2009, 267–8); on the other side of the Rhine, there was also expansion, towards the south of the plain of Baden, and in Upper Alsace, towards the north of Franche-Comté (Denaire 2009, 267–8). The Bischheim and Bruebach-Oberbergen phases each lasted some three or four generations, and were shorter than the Rössen phase. Settlements are still defined by concentrations of various small pits, and postholes have been lost to erosion; there were no substantial cemeteries, only small clusters of graves (Denaire and Lefranc 2014). From the end of the 43rd century cal BC, more than 30 BORS sites are known in Lower Alsace, spread over an area similar to that of the Rössen stage and extending over areas where both the Bischheim and Bruebach-Oberbergen styles occurred previously. Most pits attributed to the BORS II style also contain numerous Michelsberg II pots (Lüning 1968: there are no Michelsberg I pots in Alsace), indicating contact from the west (Jeunesse *et al.* 2003). This seems to me to support a ‘low’ rather than a ‘high’ chronology for the emergence of the Michelsberg culture as a whole, but this is again another obvious target for future research.

So in contrast to the passivity of traditional chronological charts or the smoothed transitions produced by summed probability distributions, the more precise date estimates offered by formal modelling in combination with rigorous seriation and typology revealed a Neolithic world in Lower Alsace busy with comings and goings, tinkering and adjustments, and relocations and realignments. More precise date estimates show that being busy was not always conducted at the same tempo, though we think most of the changes would have been perceptible within a lifetime scale, and many of them within one to two generations.

Lower Alsace is just one of innumerable regions across Neolithic Europe, and it does not necessarily speak for them all. Our study indicates, however, what could be achieved for the broader panorama, if a similar methodology were applied elsewhere. One could start with Upper Alsace and the middle Rhine, and move out to the Neckar valley, south-west Bavaria and the valleys of eastern France. I will

refer to other regional studies carried out in the ToTL project, in the Polish lowlands, in Transdanubia or western Hungary, and across the distribution of the Vinča culture in south-east Europe. But next I want to draw on a different kind of Neolithic archaeology, but in which it has also proved possible to discern varying patterns in the lives of settlements and another unexpected discontinuity.

Another study of twists and turns in settlement trajectory: Late Neolithic Orkney

Neolithic Orkney is well known for its rich and well preserved remains, and World Heritage status was given in 1999 to an area of western Mainland with a significant concentration of outstanding settlements and monuments. While there are plenty of features cut into the subsoil, including the fairly recent detection of timber-framed houses (Richards and Jones 2016), here is an archaeology dominated by stone, in the form of house walls, cairns and their chambers, and circles of standing stones. Even more excitingly, new discoveries continue to be made in the archipelago, such as of the settlement of Braes of Ha'Breck, on the small island of Wyre (Thomas and Lee 2012). Both Barnhouse (Richards 2005) and Ness of Brodgar (Towers *et al.* 2015; Card *et al.* 2017) (Fig. 4.4) were comparatively recent discoveries, the latter indeed being found after the designation of the Heart of Neolithic Orkney, and the word is that there appears to be another major settlement concentration a little to their north-west, at Bookan (Chris Gee and Colin Richards, *pers. comm.*). Given these continuing tales of the unexpected, any account of the development of the sequence on Neolithic Orkney may seem premature, but a series of changes in our understanding have taken place, to which the ToTL results add another twist.



Fig. 4.4: Top: Barnhouse House 2 (photo: Colin Richards); below: Ness of Brodgar (photo: Hugo Anderson Whymark).

In very brief outline (and for a more detailed account, see Bayliss *et al.* 2017), previous research had identified different categories of tombs and material, especially pottery (Davidson and Henshall 1989; MacSween 2007), and a number of settlements with stone-walled houses. A basic distinction was made between an earlier and a later Neolithic, the latter with features perhaps suggestive of a more stratified society (Renfrew 1979). Modest numbers of radiocarbon dates had been obtained, and it was believed that the plateau on the

late fourth millennium calibration curve prevented any reliable differentiation in the dating of sites falling in that period, and perhaps even as late as 2900 cal BC (Ashmore 1998, 142). Intensified research since, refinement of the calibration curve, technical advances including the ability to date calcined bone, and many more radiocarbon samples have all combined to help to construct a more nuanced sequence overall (Richards and Jones 2016; Griffiths 2016; Bayliss *et al.* 2017).

My principal interest here is in the Late Neolithic, from the last two centuries or so of the fourth millennium cal BC to the middle and later part of the third, but we should note the new evidence for the start of Neolithic activity on Orkney probably no earlier than the 37th century cal BC (Griffiths 2016, 296; Bunting *et al.* in prep.), consistent with the picture outlined for Scotland south of the Great Glen (Whittle *et al.* 2011, chapter 14). The evidence now suggests small-scale dispersed settlements, with timber buildings, in the earlier to mid-fourth millennium cal BC, and with round-based pottery; less elaborate forms of chambered tombs may belong here, possibly including some stalled cairns. Thereafter come a series of important changes, with stone houses making an appearance, and architecturally more elaborate and ambitious monuments, including the Maeshowe passage graves or tombs (in alignment with the terminology of the Irish monuments with which they appear to be closely linked) and large stalled cairns. On present indications, the novel flat-based ceramic, Grooved Ware, emerged in the 32nd century cal BC (MacSween *et al.* 2015; Richards *et al.* 2016; Bayliss *et al.* 2017; Sheridan *et al.* in prep.). Significant settlements with Grooved Ware such as Barnhouse (Richards *et al.* 2016) help to produce that date, and Ness of Brodgar, though not yet systematically investigated at lower levels, could also begin then or soon after (Card *et al.* 2017). Though not all of them can yet be dated with precision (a crying need for future research), significant monuments in this part of western Mainland have also been assigned to this Late Neolithic period, including the Maeshowe passage tomb, and the circles of the Stones of Stenness, in use by the 30th century cal BC (Bayliss *et al.* 2017), and the Ring of Brodgar, on the basis of OSL dates² in the mid-third millennium (Downes *et al.* 2013) (Fig. 4.5). Initial dating of the complex of buildings and structures at Ness of Brodgar was taken to suggest a long history of use, well into the later third millennium cal BC (Towers *et al.* 2015).

So a more detailed sequence has been emerging, but still with a strong sense of continuity. Important possible implications have been explored. Colin Richards and Richard Jones (2016) have seen in the

later fourth-millennium appearance of stone buildings an emphasis on both the physical form and the concept of the house, drawing on the idea of Lévi-Strauss of ‘house societies’. They observed that while it had often been assumed in general that houses served in various ways as the model or template for the development of tombs, and thus that stalled cairns in particular would have been preceded by stone houses, in their understanding the sequence had been reversed (Richards and Jones 2016, 40).



Fig. 4.5: Top: Ring of Brodgar; below: the Stones of Stenness (photos: Colin Richards).

In the ToTL project, we decided to concentrate on Late Neolithic settlements associated with Grooved Ware, and have helped to refine the dating of the individual sites of Pool on Sanday (MacSween *et al.* 2015), Links of Noltland on Westray (Clarke *et al.* 2017), Barnhouse (Richards *et al.* 2016), Ness of Brodgar (Card *et al.* 2017) and Skara Brae (Clarke and Shepherd in prep.). Complementing the modelling of the earlier part of the sequence (Griffiths 2016), we also rounded up the available dates for later fourth and third millennium activity on Orkney, making use of 613 radiocarbon measurements (plus 79 luminescence ages) from 31 sites (Bayliss *et al.* 2017) (Fig. 4.9). Without doubt, further research and dating will refine (and potentially subvert) the emergent picture, but two trends in particular have been surprising.

First, there is a period in the later fourth millennium of significant overlap. I will discuss tombs and material culture in more detail later in this chapter, but our models indicate that both stalled cairns and Maeshowe passage graves were first constructed in the middle centuries of the fourth millennium cal BC, although on the basis of current evidence it is not possible to say which came first; human remains may have been deposited in Maeshowe passage graves into the middle centuries of the third millennium, whereas the primary deposition of human remains in stalled cairns appears to have ended in the first quarter of the millennium (Bayliss *et al.* 2017). Though timber buildings may have appeared first, both timber and stone houses were in use during the second half of the fourth millennium cal BC (Bayliss *et al.* 2017). And finally, it is clear that both round-based and flat-based pots were in use together in the centuries around 3000 cal BC, probably for a couple of centuries or more (Sheridan *et al.* in prep.). It had already been suggested some time ago that such differences in tombs and material could be down to regional variation within the archipelago, with greater innovation in the heart of Mainland (Davidson and Henshall 1989, 90–3). At the very least, as in the Lower Alsace study, we need to pay attention to the details of both space and time.

Secondly, we were able to go beyond our estimates for the timing and duration of individual sites. Individual sites had particular durations (which I also note in the next section), and we obviously knew that there were hiatuses in the occupation of individual sites like Pool (MacSween *et al.* 2015) and Skara Brae (Clarke and Shepherd in prep.). Wider modelling, however, provided a formal estimate of the intensity of settlement activity in ‘core’ and ‘peripheral’ areas across Orkney from 3500–2200 cal BC, derived from estimates of the number

of structures in use on individual sites in 25-year periods (e.g. Richards *et al.* 2016, fig. 14; Bayliss *et al.* 2017, fig. 5) (Fig. 4.6). The intensity in activity in the core, defined as the concentration of sites and monuments in the Stenness-Brodgar area, from c. 3125–2850 cal BC occurs in tandem with the start of a general decline in the periphery, defined simply as the rest of the archipelago, with a clear lull in settlement intensity apparent in the 28th century cal BC. Although settlement in the periphery appears then to recover to its early intensity, in the 27th–26th centuries cal BC, the core shows no similar resurgence, before peripheral settlement intensity goes into a second major decline in the later part of the millennium, with a final abandonment of villages probably in the 24th century cal BC (Bayliss *et al.* 2017). Unlike in the Lower Alsace study, we have not proposed complete hiatus. Here, our suggestion is that local political tensions and social concerns driving the trajectory towards closer settlement nucleation could simply not be sustained, despite people having invested time, labour and efforts in monuments and connections that stretched well beyond Orkney. Life on the ground always has a social and political context.

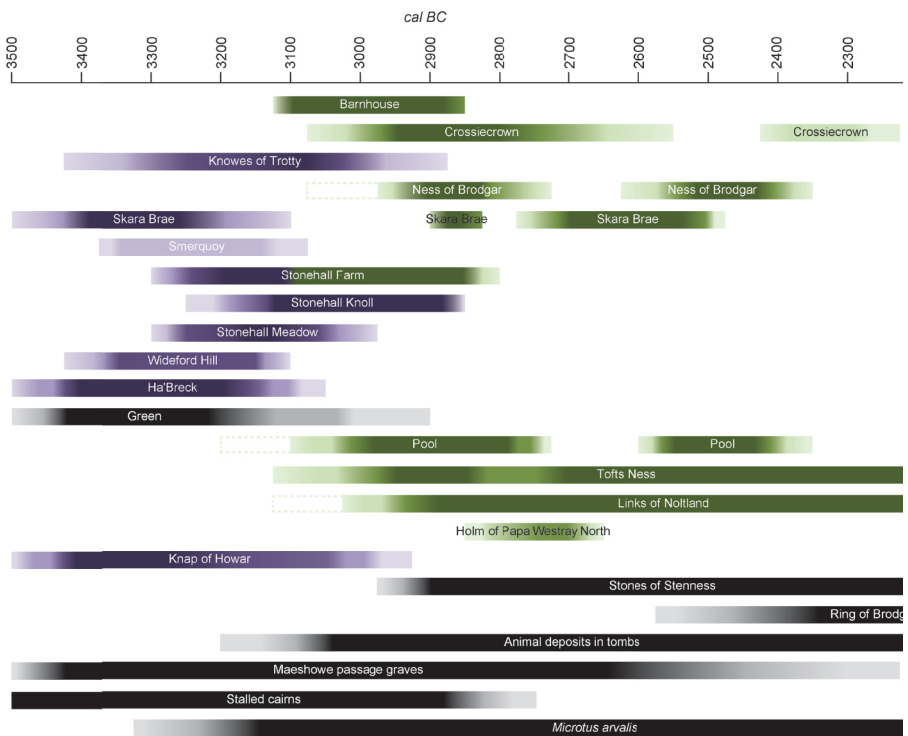


Fig. 4.6: Summary of formal modelling for the Neolithic sequence in Orkney in the later fourth and third millennia cal BC. Mauve: associated with round-based pottery; green: associated with flat-

Site biographies

We can think about this further by looking at the range of site durations which formal modelling is beginning to provide, and reflecting on the varying human scales already outlined in [Chapter 2](#). Examples considered so far include the long-lived tells of Vinča-Belo Brdo and Uivar noted in [Chapter 1](#), the brief occupations of many Alpine foreland settlements discussed in [Chapter 3](#), and the generations-long biographies of LBK and other Danubian sites in Lower Alsace and elsewhere. Further cases add to the sense of diversity. There was no one set way of living together.

It might seem from the list above that such diversity is culturally or temporally patterned. If the occupation of place was conditioned by the values attached to a sense of community, as we have argued for the Alpine foreland (Hofmann *et al.* 2016), there is little support for any notion of rigid or prescriptive rules governing durations. Beyond Vinča-Belo Brdo and Uivar, there are so far very few other tells in south-east Europe with formally modelled chronologies, but even these hint at variation. On the basis of a rather small number of samples, including potentially residual material, the timing and span of the up to 3-m deposit at Okolište, Bosnia (Müller *et al.* 2013; R. Hofmann 2013, 49), seem broadly comparable to those already noted at Uivar. While the overall span of the 3.5-m tell deposit at Polgár-Csőszhalom, Hungary, also appears similar, there are indications of variation in the intensity of its use, with a lot of activity concentrated in the latter part of its use, from the 47th to the 45th centuries cal BC (Raczky *et al.* 2011, 58; 2015, 43–4), and we know that this was an unusual accumulation, with rondel-like ditches, and a series of other distinctive features of layout and deposition (Raczky *et al.* 2011). The tell deposit at Pietrele, Romania, is up to 9 m deep, and from the portions so far excavated, and dating from the mid-fifth millennium cal BC onwards, a much faster rate of accumulation is implied than for Belo Brdo and others (Reingruber 2015a); the burning of two-storey buildings and substantial amounts of imported clay and earth may be implicated (Agathe Reingruber, *pers. comm.*). Likewise, while many of the Alpine foreland sites have the strikingly short biographies sketched in [Chapter 2](#), the example of Sipplingen on the Bodensee shows longer durations (Billamboz 2014a), and if the several individual archaeological sites around the head of Lake Zürich are taken as one locale, this is another impressive example of continuity across the fourth millennium (Bleicher 2009b; 2015; Bleicher and Burger 2015).

Further examples from the ToTL project provide other estimates for site durations somewhere in between those of long-lived tells and short-lived occupations in the Alpine foreland. The large longhouse settlement of Szederkény-Kukorica-dűlő in south-west Hungary probably lasted overall from the later part of the 54th century cal BC to the first decades of the 52nd century cal BC (Figs 4.7–8). Three foci could be seen within the site as a whole, and formal modelling suggests that occupation probably began at the same time in the eastern and western parts, but started a decade or two later in the central part, while the western part was probably the last to be abandoned (Jakucs *et al.* 2016). It belonged to a period of significant cultural change and probable population expansion. In the Polish lowlands near Poznań, the late longhouse settlement of Racot 18, belonging to the Late Lengyel culture (see Fig. 4.15), has a formally estimated span from probably the late 44th century cal BC to the early 40th century cal BC (Czerniak *et al.* 2016). Again this is a period of settlement expansion. Jumping again further north, various estimates from the ToTL modelling on Orkney suggest that some site durations of two centuries or more were not uncommon in the context of the Late Neolithic, as seen at Barnhouse, Ness of Brodgar, Pool and Skara Brae (Richards *et al.* 2016; Card *et al.* 2017; MacSween *et al.* 2015; Clarke and Shepherd in prep.; Bayliss *et al.* 2017). We cannot yet establish the precise relationship between Barnhouse and Ness of Brodgar, since the lower levels of the latter have not yet been fully examined, let alone dated and modelled. We have argued that Barnhouse can be seen in its local setting as a deliberate foundation in an area of western mainland previously perhaps relatively sparsely occupied (Richards *et al.* 2016, 217–18). In all three situations, it would seem to have made sense for communities to have established their position, in shifting circumstances, by prolonged occupation of place.

The most striking marking of long occupation was visible in the form of substantial tells, and the tell deposit at Vinča-Belo Brdo is underlain by Starčevo features, such that the place was used from the earlier sixth millennium to the mid-fifth millennium cal BC, albeit with an apparent gap between the Starčevo and Vinča culture phases. There are other examples of comparable longevity which were not marked by tell formation. A strong candidate for the most impressive example of this is the site of Alsónyék-Bátaszék in south-west Hungary. This was used overall from c. 5750 cal BC to c. 4300 cal BC. That encompasses a succession of Starčevo, LBK, Sopot and Lengyel occupations, spread horizontally over wide areas of the site, and

without formation of deep stratigraphy (Oszrás *et al.* 2016a) (Fig. 4.9). There appears to have been a gap between the Starčevo and LBK occupations, but probable overlap between the use of the LBK and Sopot settlements, just over a kilometre apart. Each of the individual phases lasted in the order of centuries (Bánffy *et al.* 2016). Such persistence of place could be because of the strategic location of the site just west of the Danube, linking the Carpathian basin and Transdanubia, but the circumstances of each occupation could have been different. So far, Alsónyék is by far the largest Starčevo settlement known in Transdanubia, matching those known to its south and south-east in Croatia and northern Serbia, and its two-century long occupation was plausibly part of a strategy for the colonisation of this territory by expanding farming populations (Oross *et al.* 2016a). The succeeding LBK settlement was in use for some three centuries of more, involving a concentration of around 50 longhouses, clearly not all in use at the same time (Oross *et al.* 2016b). This relatively modest-sized LBK occupation was just one element in what appears thanks to intensified research over the last few decades to have been an explosion of LBK settlement across Transdanubia as a whole (and of course beyond, to many points north, west and east) (Marton and Oross 2012). The Sopot occupation may also have lasted two or three centuries, as part of a complex series of cultural changes and possible population changes (Oross *et al.* 2016c).

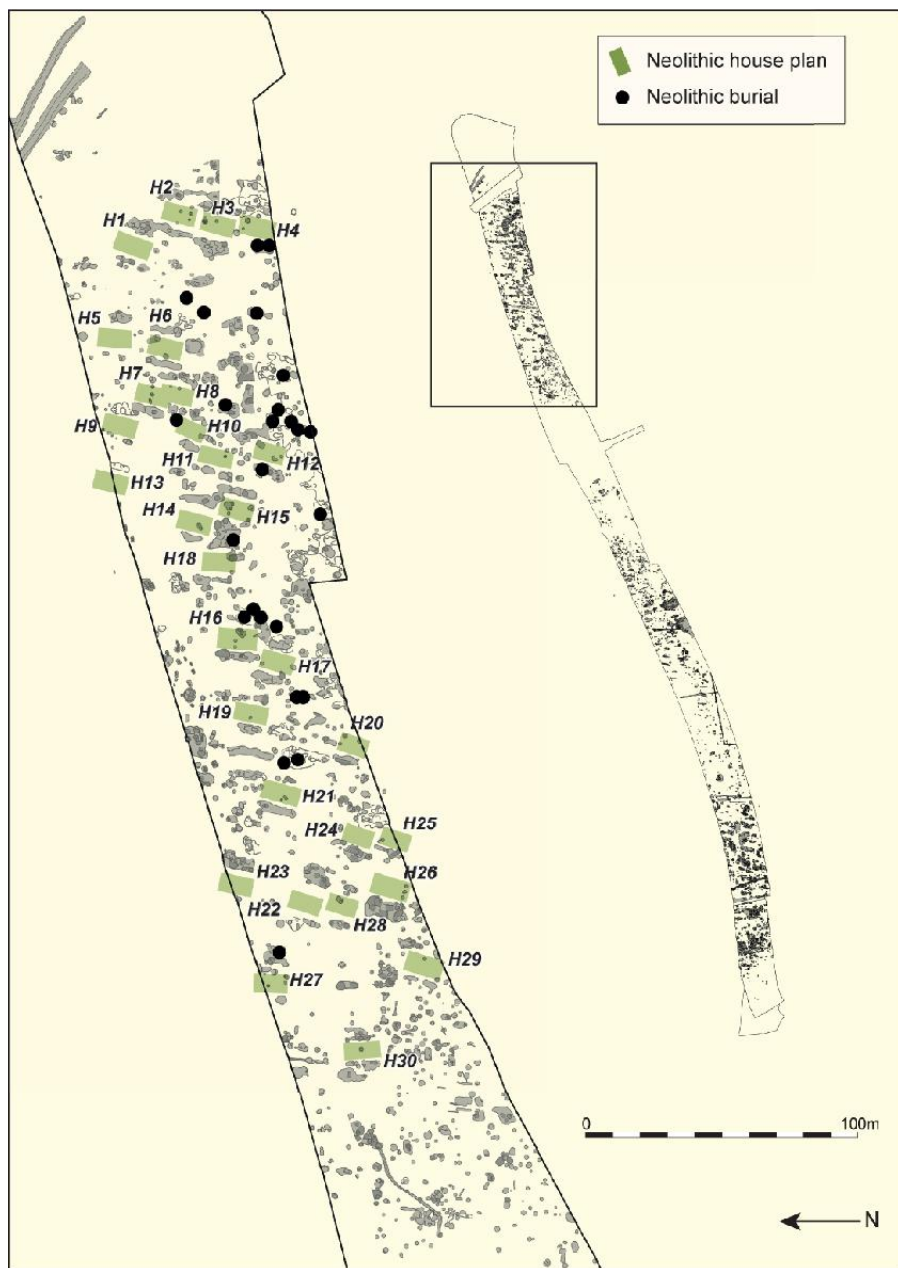


Fig. 4.7: Plan of the eastern part of the settlement at Szederkény. After Jakucs et al. (2016).

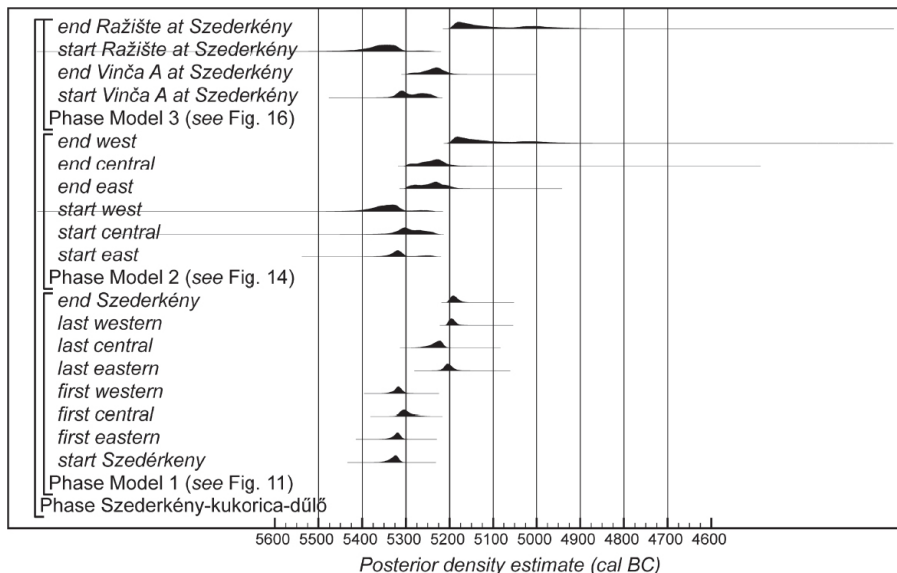


Fig. 4.8: Model for the timing and duration of the Szederkény settlement. After Jakucs et al. (2016).

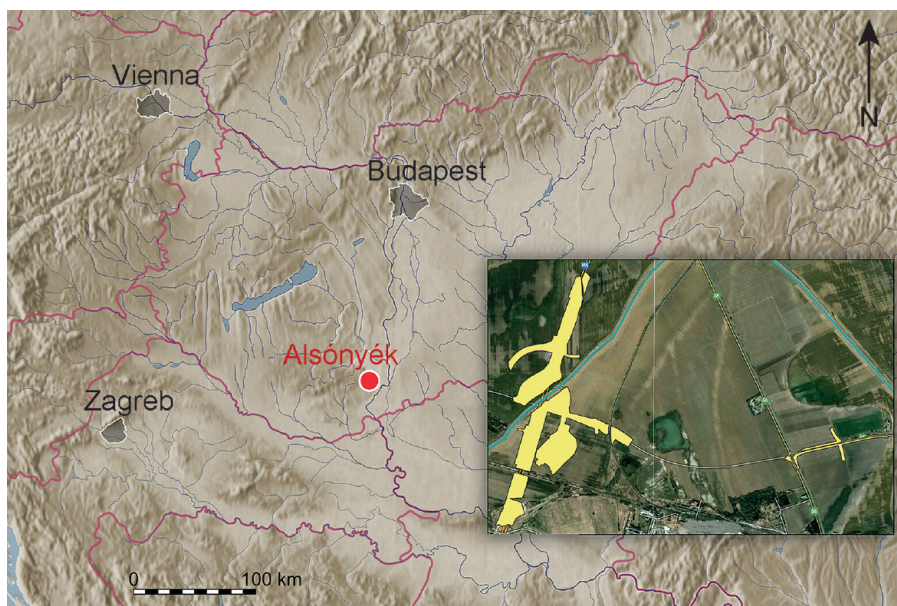


Fig. 4.9: Location and outline plan of the excavations at Alsónyék. After Osztás et al. (2016a).

In this part of Europe, there is no sign of post-LBK hiatus as discussed earlier for further west. At Alsónyék, the Sopot occupation was followed, possibly with some overlap, by the much larger Lengyel settlement. This was a major flat settlement, probably covering up to 80 hectares, and in the excavated slices excavated in advance of road construction comprising over 120 known houses, numerous pits and

intersecting pit complexes, and some 2300 individual graves (Osztás *et al.* 2016b) (Fig. 4.10; see also Figs 5.6–5.7). When these numbers are scaled up to match the whole extent of the site, it is likely that we are dealing with hundreds of houses and a remarkable total of thousands of burials (Bánffy *et al.* 2016). The buildings were not as big as large LBK longhouses, but were nonetheless substantial structures between 14 m and 22 m long, with two or three rooms (and perhaps lofts). These share broadly the same orientation and are spread fairly evenly across the excavated parts of the site, in what may be rows and perhaps forming a series of small neighbourhoods; there are no obvious central areas, or unusually large buildings that stand out from the rest (Osztás *et al.* 2016a, fig. 7). That impression is reinforced by the distribution of the pits and pit complexes around the houses (Osztás *et al.* 2016b, figs 8–10), and by the layout of the individual graves. Some of these are scattered across the site seemingly on their own, but the great majority lie in smaller and bigger clusters or groups, the largest comprising over a hundred burials. Within the grave groups, the burials generally share the same orientation, and the same preferences for body position (Osztás *et al.* 2016b, figs 2–6). There was an impressive range of pottery, stone tools, ornaments in variety of materials, and copper, and there was variation both in the treatment of the grave pits themselves and the abundance with which the deceased were provided with grave goods.

More than 200 samples were radiocarbon dated for the Lengyel phase at Alsónyék. Overall, activity on the site is estimated to have lasted for some five centuries, probably from the 48th century cal BC to the 44th or 43rd century cal BC (Osztás *et al.* 2016b). That conforms with a normal expectation that such a large site would have a long history. But what the extensive dating programme and the formal modelling of results showed was something much more differentiated. For the purpose of analysis, three subsites within the main excavated areas can be distinguished: 5603, 11 and 10B. These were modelled separately. The modelled estimates are precise enough that it is possible to estimate robustly the timing of activity across the site, and here again it is worth relating some of the detail of the results.

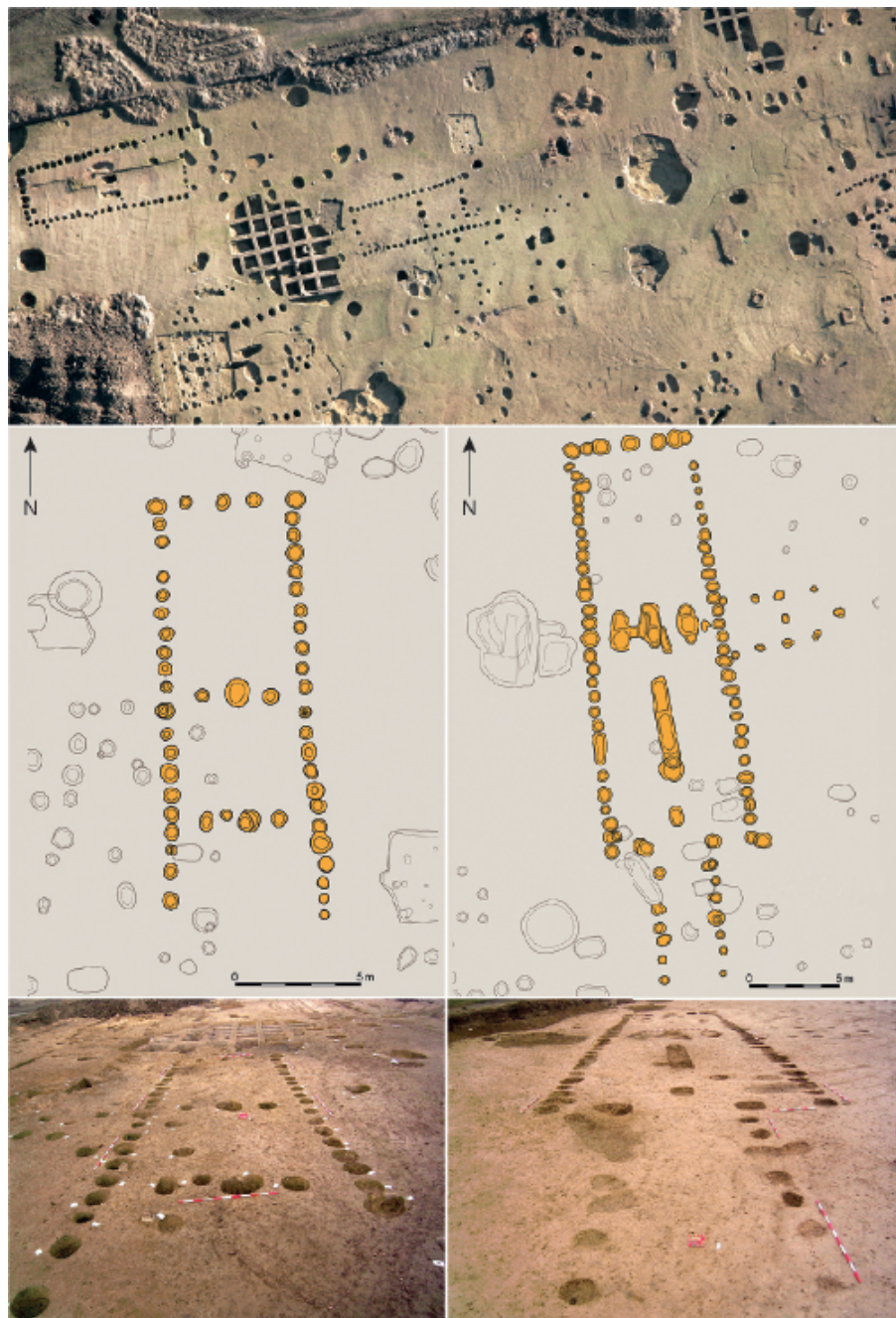


Fig. 4.10: Lengyel houses and features at Alsónyék. After Osztás et al. (2016b).

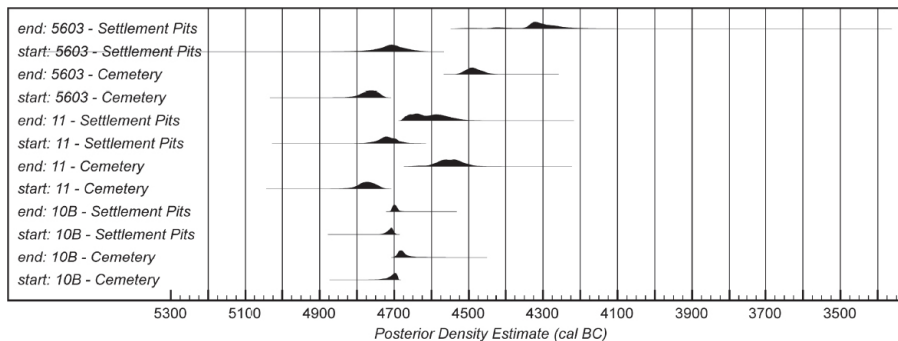


Fig. 4.11: Model of the timings and durations of settlement and burial at Alsónyék in the Lengyel phase. After Osztás et al. (2016b).

The formal modelling³ suggests that burial activity in subsites 5603 and 11 probably began in the 48th century cal BC, and on subsite 10B at the turn of the century around 4700 cal BC (Fig. 4.11). From the late 48th century and at its turn, settlement was established across a wide area, in subsites 11, 5603 and 10B. After a brief episode of intense occupation, lasting at most a few decades, settlement and then burial ended on the northernmost subsite 10B, in the 4700s or 4690s cal BC and early 47th century respectively. Settlement also ended before burial on subsite 11, but endured for much longer. Both settlement and burial endured longest on subsite 5603, although here the end of burial preceded the end of settlement by well over a century. Burial ended here in the earlier 45th century cal BC), and settlement in the 44th–43rd centuries cal BC.

From these estimates, we can see a much more complex settlement history. It appears that the first activity was burial, the importance of place being marked at this point by commemoration of the dead. Then settlement followed. But the whole or the greater part of the site was in its most intense use, for both occupation and burial, for only a short period, to be measured in decades or over perhaps only the generation or two who lived in the mid- to later 48th century cal BC (Bánffy *et al.* 2016, 300). So within the impressively large Lengyel settlement of Alsónyék and contained within its longer history there is a striking story of much more rapid growth, followed by a long decline.

We were able to go further. The formal modelling approach enabled further calculations about the probable intensity of activity through time, at 25-year intervals, seen in the estimated numbers of features including houses and of deaths through the long overall history of Alsónyék (Figs 4.12–4.14). That involved a series of assumptions, which are set out in detail in our synthesis paper (Bánffy *et al.* 2016, 298–305). First, we had to scale up the number of features and structures, going from the excavated areas to the site as a whole. For

example, just over 120 Lengyel houses were excavated, but it is estimated that some 400 houses were used over the span of the Lengyel phase at Alsónyék (Bánffy *et al.* 2016, 302). For the numbers of the dead, we had to take into account the probable under-representation of children in the mortuary record of the site (that is, a higher mortality rate for children is probable, on all known analogies, than is actually found in the recovered evidence from Alsónyék), and so concentrated on the adults. Their numbers too were scaled up by area, and adjusted for the absence of children, and population size was then calculated using varying estimates of mortality (for example 30–40 deaths per annum per 1000 population in normal mortality conditions: Acsádi and Nemeskéri 1970) (Bánffy *et al.* 2016, 300, figs 8–9). Alternatives were considered and compared, to try to get at the numbers of people living on the site at any one time, first by using estimates of life expectancy, and again correcting for the under-represented children, and secondly, following Ubelaker (1999), by factoring in mortality rate and duration, once again taking account of the children, to get an average estimate of the population who lived in different parts of the settlement (Bánffy *et al.* 2016, 301). The three methods did not produce identical results, but they did give broadly comparable answers, such that we can think of well over 2000 people living in sub-site 10B in the generation before 4700 cal BC (Bánffy *et al.* 2016, 302). We also estimated the possible number of houses in use in any 25-year interval, tentatively using the median value of a 25-year duration for houses on other broadly contemporary sites in the Carpathian basin (Bánffy *et al.* 2016, 303–4, fig. 10). This suggests over 200 houses in the generation before 4700 cal BC, for example, but fewer than ten in the generation before 4500 cal BC (Bánffy *et al.* 2016, 304, fig. 11).

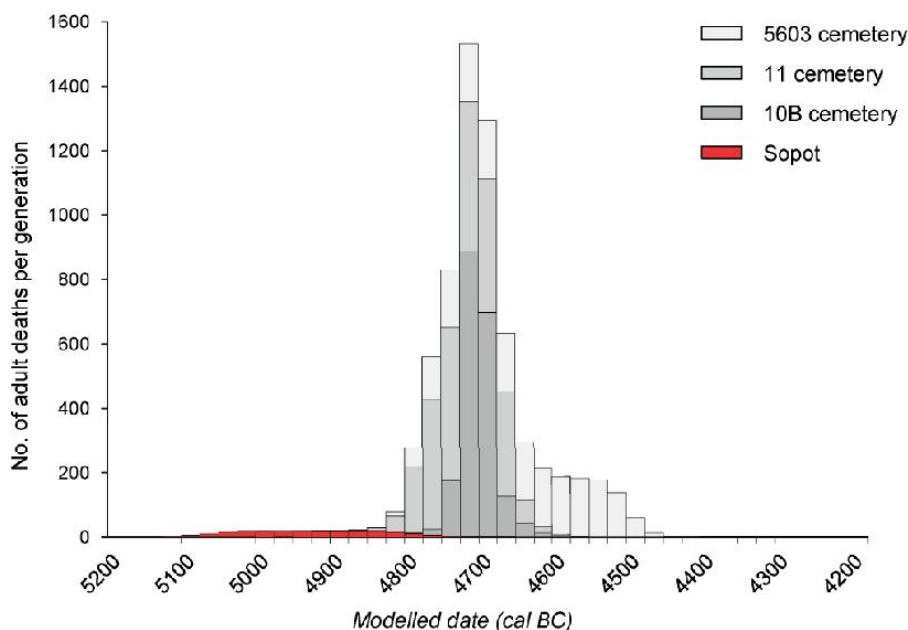
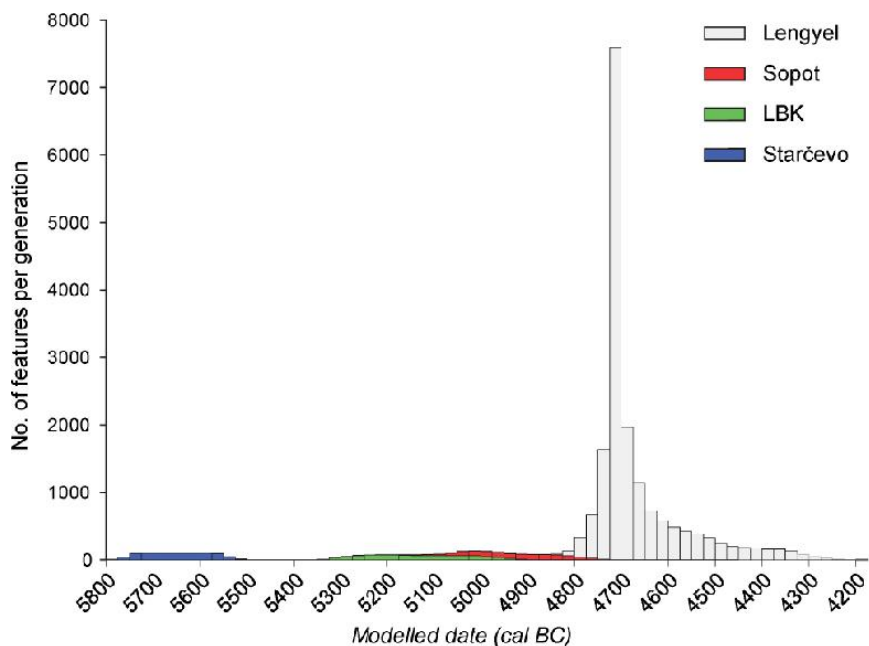


Fig. 4.12: Modelling population numbers for Lengyel Alsónyék. Top: estimated number of features per generation, right through the site sequence; below: estimated number of adult deaths per generation, focusing on the Sopot and Lengyel phases. After Bánffy et al. (2016).

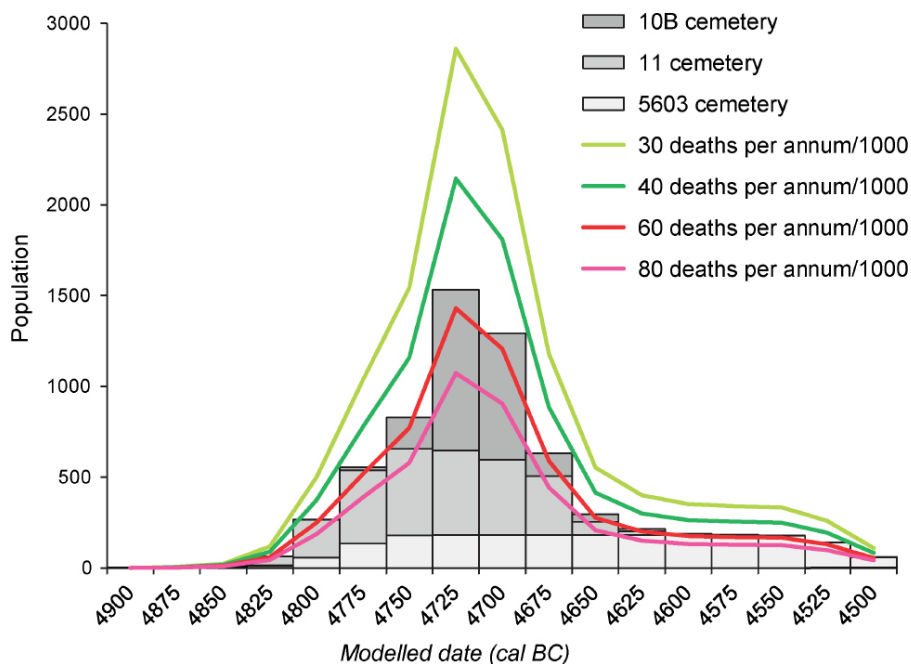


Fig. 4.13: Modelling population numbers for Lengyel Alsónyék. Estimated population of the Lengyel community through time, including children, at various mortality rates. After Bánffy et al. (2016, 300–302).

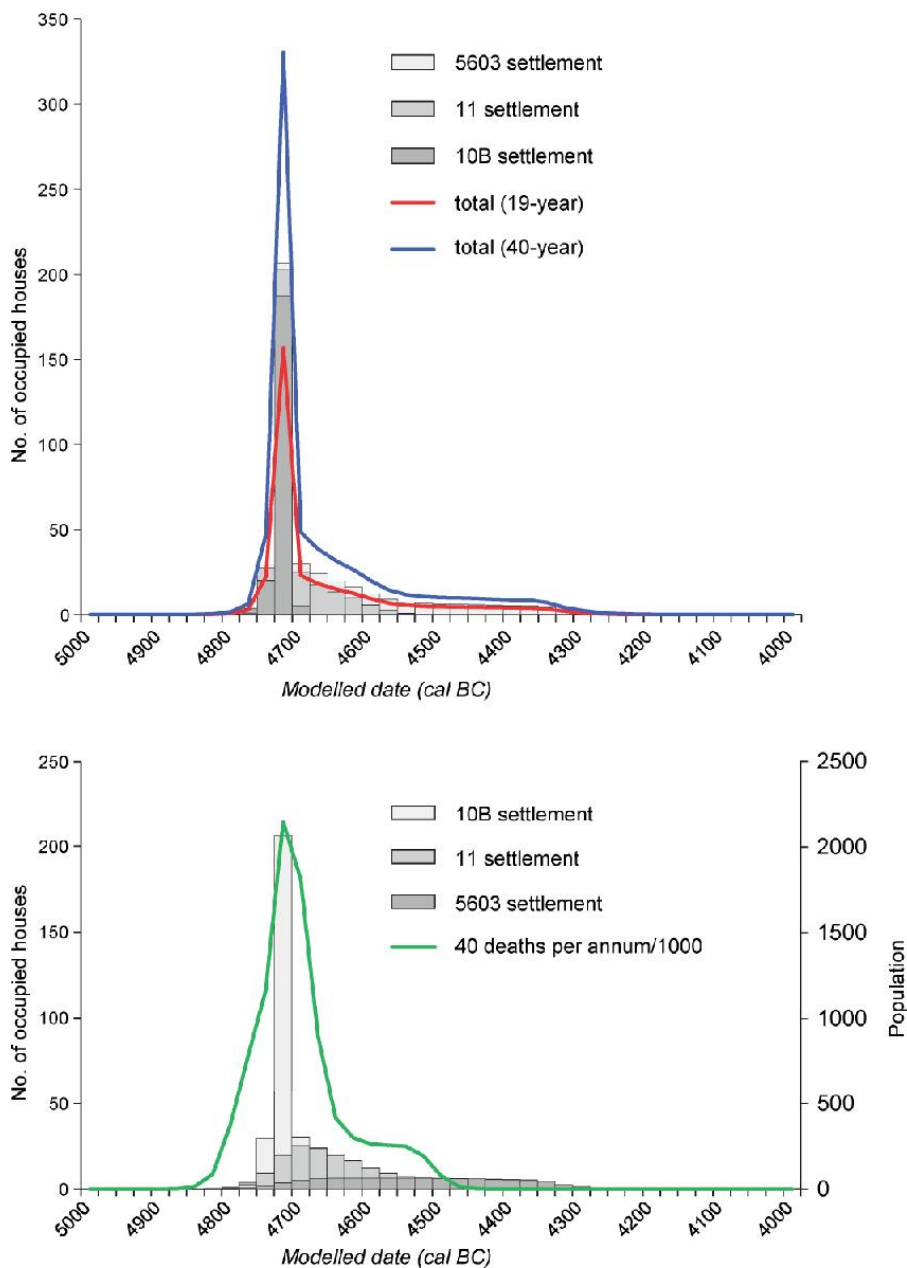


Fig. 4.14: Modelling population numbers for Lengyel Alsónyék. Top: estimated numbers of occupied houses per 25-year generation; for details of 19-year and 40-year house durations, see Bánffy et al. (2016, 303). Below: estimated numbers of houses occupied per generation in the different subsites, and estimated population (green line) using the figure of 40 deaths among 1000 people (including children) per generation. After Bánffy et al. (2016).

These are complicated estimates, and of course they use various assumptions. They are, however, explicit and quantified, and use the direct evidence of the site, in contrast to those other estimates of

population (already briefly referred to in [Chapter 1](#); see further discussion in [Chapter 6](#)) which simply attempt to derive population from the proxy of summed probability distributions. Overall, we concluded that the more detailed history of the Alsónyék settlement began, following previous Lengyel burials, with the establishment of a small occupation in the earlier 48th century cal BC, with a population of about 100 individuals, constituting a handful of households. In the decades before 4700 cal BC, this settlement exploded to more than 200 houses, with a population of perhaps 2500 individuals. The following century saw rapid decline, with the settlement at around 4600 cal BC containing now only around 15 houses, perhaps inhabited by around 350 people. The decline went on and by about 4500 cal BC, the population had shrunk to around 200 individuals who lived in under ten houses. By around 4400 cal BC, the settlement had contracted even further to fewer than a hundred people in a handful of households. This is not yet as precise as the dendrochronology discussed in [Chapter 3](#), but surely raises exciting prospects for future research: imagine if we had comparable information for a series of contemporary sites in a region like that around Alsónyék.

So Alsónyék offers strikingly varying tempo and durations within an overall long occupation. To complete this review of the outcomes of formal modelling of settlement durations, one more study indicates a short lifespan. Versend-Gilencsa is another substantial settlement in south-west Hungary, not far from Szederkény (Jakucs *et al.* 2018) ([Fig. 4.15](#)). It has substantial longhouses, and like Szederkény a range of pottery, including material representing early Vinča, early LBK Bicske-Biňa, the LBK Malo Korenovo variant and Sopot-Ražište styles, variously distributed in the two main parts of the site. The eastern part was the better preserved, and where our dating study was concentrated. Here there were the remains of 21 longhouses, unequivocally arranged in rows. Some time-depth is indicated by one or two overlaps. Now while the *Hofplatzmodell* or independent farmstead model has classically proposed a duration of some 25–30 years for the lifespan of individual LBK longhouses (summarised in Zimmermann 2012), the rival row theory has mooted potentially much longer durations, up to 75 years or more (Schmidt *et al.* 2005, 162; Rück 2009; 2012), but in fact neither position has yet been further tested. The results of our modelling were therefore both surprising and significant. A number of individual burials were dated, and this activity probably spanned a long period from the 53rd century cal BC to the turn of the millennium (Jakucs *et al.* 2018, figs 5–6). But the Versend settlement, on the basis of samples from the pits

flanking longhouses, belonged to the late 53rd century cal BC (Jakucs *et al.* 2018, figs 5–6). Our formal estimate for its duration was probably 10–35 years (68% probability), and given the short overall duration of the settlement, most houses were probably in use for no more than a decade or two (Jakucs *et al.* 2018, fig. 8) (Fig. 4.16).



Fig. 4.15: Plan of the longhouse rows in the eastern part of the settlement at Versend. After Jakucs *et al.* (2018).

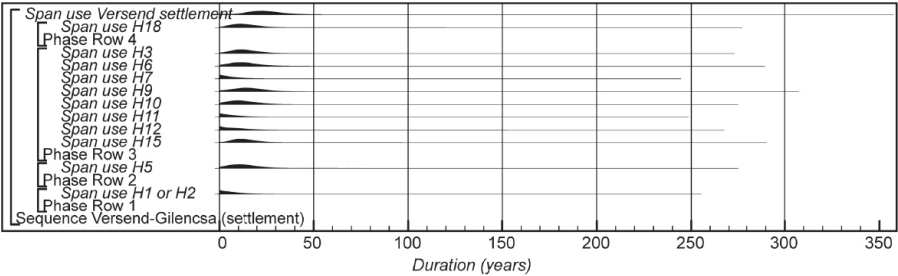


Fig. 4.16: Model for the short duration of longhouse lives at Versend. After Jakucs *et al.* (2018).

The results for Versend appear to contradict the temporality of both the *Hofplatzmodell* and the row model. But they are closer to the kind of turnover of settlements which came out of the Lower Alsace study, described above. Living in rows must have formed a very intimate kind of community, with the long sides of houses close to their neighbours and the fronts of the buildings looking directly across what were probably quite narrow lanes at the corresponding row of

structures. It may be relevant to think of comparative evidence of these kinds of conditions. The historian Keith Thomas (2009, 177) has referred to the ‘constant invigilation by the community’ in 17th-century villages and towns, and quotes (K. Thomas 2009, 27) the warning of a Jacobean cleric: ‘desire not to be singular, nor to differ from others, for it is a sign of a naughty spirit, which hath caused much evil in the world from the beginning’. As discussed also for Szederkény, the range of material at Versend does seem to project some degree of difference, but whereas the former settlement endured for generations, that of Versend did not. While we do not know the specific conditions in which Versend came to an end, perhaps the potential tensions entailed by a combination of proximity and ‘singularity’ were one contributing factor.

The lives of houses: some comparisons

Versend and other modelled examples of house duration enable some wide-ranging comparisons (Bayliss and Whittle in press). We have already seen generally short lives for houses in the Alpine foreland. The settlement at Hornstaad-Hörnle IA, for example, was burnt down in 3910 BC, a mere seven years after the first timber was felled for its construction. Although there was some rebuilding after the fire, overall the settlement appears to have been occupied for little more than 15 years (Billamboz 2006). This kind of house biography is found over and over again in the Alpine foreland, but is it confined to that region? The evidence suggests not. Examples from the tell sites of Vinča-Belo Brdo, Uivar and others (Tasić *et al.* 2015; 2016a; 2016b; Draşovean *et al.* 2017; R. Hofmann 2013) also show short-lived houses, and many Neolithic adults would surely have lived in more than one house over their lifetime. This active renewal presumably contributed not only materially but also symbolically to the sense of continuity ever growing in tell formation. But the tells also show more variability, with a number of structures remaining in use for several decades (Tasić *et al.* 2016b; Draşovean *et al.* 2017), although durations of more than half a century seem to be exceptional. Uivar is particularly revealing (Draşovean *et al.* 2017), and I come back to that site in [Chapter 6](#).

Beyond tells, it is often not possible to date the duration of individual houses precisely, but rather house duration has to be inferred from the period when a particular site, or form of structure, or ceramic tradition was in use. The settlement in sub-site 10B at Alsónyék-Bátaszék, for example, lasted only a few decades (as detailed above), such that its houses can only have been occupied for a similarly restricted period. Early Neolithic timber houses in Ireland

occur for only a few generations in the 37th century cal BC and with one possible exception their plans are never superimposed (Smyth 2014, 25–6), and so this currency provides a maximum duration for any individual house. At Racot in the Polish lowlands (Fig. 4.17) the substantial long houses can be associated with a series of pottery styles, and house durations appear to range from a few decades (evidenced by successive houses within Ceramic Phase IIB) to a century or more (Phase IIIB-2; Czerniak *et al.* 2016). In the first phase, in Late Lengyel IIB, each house appears to have existed for a few decades and then to have been rebuilt in close proximity to its predecessor, which has made them look like paired houses. The last phase probably endured for more than a century (Racot IIIB-2). Orientations became more random, and successive houses were no longer built next to one another. They were also more solidly constructed and had small rooms or annexes attached (Houses 12 and 106; probably House 211). There are probable changes in associated patterns of meat-eating or feasting (Czerniak *et al.* 2016). That the longest-lasting structures here appear also to have been the most substantial, could have been part of a strategy, we have suggested, to establish more individual identities, perhaps kin-based, within the community (Czerniak *et al.* 2016).

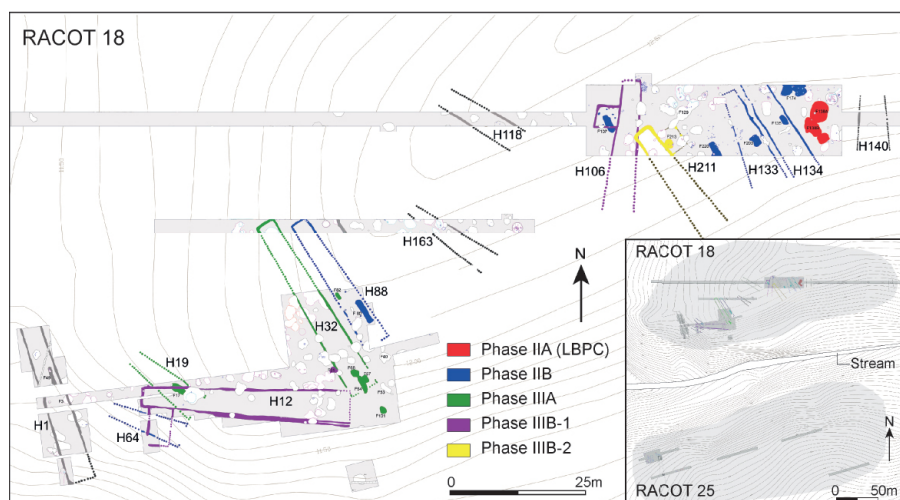


Fig. 4.17: Plan of the settlement at Racot. After Czerniak *et al.* (2016).

Barnhouse in Orkney offers further contrasts across an individual site biography. Modelling which combines the radiocarbon dates with the site stratigraphy suggests very variable house durations, ranging from a decade or two (House 11) to around 200 years (Houses 2 and

3). These structures are sub-circular and constructed from stone, not timber. Dating a century or so on either side of 3000 cal BC (Richards *et al.* 2016, fig. 6), they are also substantially later than the other houses considered here. Perhaps the more enduring architecture at Barnhouse reflects not simply the building material available, but also again the character of the household which occupied the building.

Houses were an active part of people's lives. We have often rather sloppily taken them for granted, as somehow an inevitable accompaniment to settled existence. Formal modelling as presented here (and many more examples must be another goal for future research) indicates that many houses were of short duration. But brief lives could have differing significance, depending on context: contrast tells and the Alpine foreland. When they so chose, people could also prolong house lives, as part of strategies connected to identity and self-definition.

Deaths and entrances⁴

I have already referred briefly, in [Chapter 1](#), to the formal modelling of the chronology of a handful of southern British long barrows, and have noted there how their appearance preceded that of the southern British causewayed enclosures. In this section, I am going to take a wider look at the chronology of barrows and megaliths on the one hand, from the fifth to the third millennia cal BC, and of enclosures on the other, principally those with interrupted ditch systems, belonging in the main to the later fifth and fourth millennia cal BC. This largely takes us to western Europe, where megaliths and related monuments, and enclosures, are a prominent feature of this chronological horizon. Many claims have been made for their central significance in Neolithic lives, worldviews, identities and social and political relationships. For both kinds of monument, I am interested here in the first place in issues of timing, order or sequence, and duration, and I want to stress how many of these questions remain unresolved; this section is more an agenda for future research than a synthesis of comprehensive new results, and my treatment will be selective. Modelling so far has shown that the lifespans of some individual megaliths, barrows and enclosures can be very brief, contrary to a widespread earlier expectation that monuments were constructions that by definition endured for significant periods of time. It should be possible, in future research, to establish with far greater precision not only the biographies of many more individual sites, but also the sequence of appearances region by region, and ultimately a much more detailed history of these phenomena as a whole. For that to be achieved, several challenges have to be overcome.

Megaliths and barrows

We do not yet know where and when the first megalithic and related constructions were made, and it remains unclear whether there was a single or multiple points of origin, and slow or rapid dispersal of one idea or many variations on a theme. To plunge in at one particular point, modelling in Britain and Ireland suggests that the first appearances probably belong after 4000 cal BC, and mostly from about 3800 cal BC onwards. The megalithic chambers at Coldrum, Kent, appear to have been in use from the 40th century cal BC onwards (Wysocki *et al.* 2013), but southern British long barrows, Irish portal tombs and court cairns, portal dolmens in south-west Wales, and western Scottish Clyde cairns and eastern Scottish barrows all appear to have been built no earlier than about 3800 cal BC (Bayliss and Whittle 2007; Lynch 2014; Schulting *et al.* 2012; Cummings and Richards 2016; Whittle *et al.* 2011); megaliths on Orkney may have first been built even later (Griffiths 2016; Bayliss *et al.* 2017) and the first Irish passage tombs, despite claims that they could go back to around or even before 4000 cal BC (Sheridan 2003; 2010), do not appear to be any earlier than about 3700 cal BC, and could have started later (Berg and Hensey 2013).

Likewise, sequences of construction in northern Germany and southern Scandinavia do not appear to begin before the first centuries of the fourth millennium cal BC (Ebbesen 2007; Eriksen and Andersen 2014; Hinz *et al.* 2012; Müller and Peterson 2015), though I would not claim on the basis of existing dating that we know reliably and precisely when and where they first began in these areas. We do know that many of the collective tombs, the *allées sépulcrales* and *hypogées* in the Paris basin in northern France and passage graves of southern Scandinavia, belong much later: broadly speaking to the second half of the fourth millennium cal BC (Blin 2015).

That leaves two other possibilities for earlier beginnings: Brittany and Iberia. Sometimes extravagantly high chronologies have been proposed for the start of the Neolithic of Brittany, in the first half of the fifth millennium cal BC, and one surprising candidate for early construction, among a range of megalithic forms, from *tertres tumulaires* to passage graves, are the *grands tumulus* of the Carnac region, claimed to go back as early as c. 4500 cal BC (Cassen *et al.* 2009). The enduring challenge here, however, is to find material that securely dates monument construction, in the absence of easily identifiable early Neolithic settlements. Most dated samples are on charcoal of unknown age from old land surfaces under cairns and chambers. Jadeitite axes and rings from the cists under the *grand*

tumulus of Mont St Michel, Carnac, have been related to the long chronology proposed for jadeitite and related axes across western Europe as a whole (Pétrequin *et al.* 2012; 2017), and specifically to types which are alleged to have a currency around 4500 cal BC (Pétrequin *et al.* 2011, figs 5–6), but the dates for Brittany rely on the estimates noted above for *grands tumulus*, and those for the wider jadeitite chronology have not been formally modelled, and are not yet published in detail. Other considerations and points of reference could suggest a lower chronology for Brittany than often envisaged. Settlements of the post-LBK Villeneuve-St Germain group are now known as far west as central Brittany, and have a possible but insecurely established date range of c. 4900–4700/4600 cal BC (Dubouloz 2003); it seems unlikely that the Neolithic in Brittany begins before this. If the Neolithic in Brittany is some kind of fusion of indigenous inhabitants and expanding population from the Paris basin, then the Passy monuments, elongated barrows over single burials, may have been part of the ideas behind the creation of megaliths in Brittany; those Passy monuments are part of the Cerny phase in the Paris basin, and have been informally estimated as dating to c. 4600–4300 cal BC (A. Thomas *et al.* 2011). Much of the diagnostic material from supposed early Breton tombs belongs to what has been defined as the Late Castelletic phase, whose formally modelled date span is c. 4300–3900 cal BC (Cassen *et al.* 2009). Tumulus C at Prissé-la-Charrière in central-west France to the south of Brittany also appears to fall within that span (Scarre *et al.* 2003). So some monuments in Brittany, and others in Normandy (Marcigny 2014), do appear comfortably to predate those in Britain and Ireland, but it seems to me to remain an open question whether any are as early as even the mid-fifth millennium cal BC. Simple Bayesian models of existing, available dates have recently been published (Schulz Paulsson 2017).

The chronology of early megalithic monuments in Iberia has not yet been formally modelled in detail, though there is a better range of samples than in Brittany. Summed and informal estimates suggest the appearance of the first tombs, of quite simple megalithic construction and with collective burial deposits, no earlier than the late fifth millennium cal BC (Balseira Nieto *et al.* 2015; García Sanjuán *et al.* 2011). Simple models for a range of megalithic monuments in different parts of Iberia have also recently been published (Schulz Paulsson 2017). These appear to suggest only a broad geographical zone – from Iberia up to Brittany – within which the first megalithic monuments may have emerged in the fifth millennium cal BC (Schulz

Paulsson 2017, 312, fig. 12.1), but samples remain relatively scarce and are often far from ideal. Since there are variscite or coloured stone beads of probable Iberian origin in Breton monuments, it is possible that this is a further indication of later rather than earlier dating for the appearance of megalithic constructions in north-west France.

Even this very brief review indicates how much remains to be established in much greater detail and within a formal framework. In terms of overall narrative, it is not yet to be excluded that ideas conceived in Iberia influenced the first constructions in Brittany. Though at this stage north-west France looks to be the strongest candidate for having the earliest monuments, it remains to be seen, after much better sampling and more formal modelling, what the chronological relationship was between France and Iberia. There is a common notion (which goes back to Gordon Childe) that the idea of megaliths in general can be related to commemoration of the Danubian longhouse, though that can hardly apply to Iberia, and if it applies in Brittany it may anyway have been mediated through forms such as the Passy mounds. Late longhouses in Poland, in use at Racot for example till the late fifth or even earliest fourth millennium cal BC (Czerniak *et al.* 2016), could plausibly have been a point of reference for some of the long barrows, earthen and megalithic, of the north European plain and southern Scandinavia, but in Britain, with modelled date estimates available, there is now a gap probably of centuries between the last longhouses on the adjacent continent and the first main appearances from the 38th century cal BC onwards. This means we have to rethink notions of origins. Perhaps local houses were the inspiration for some of the first mounds and cairns. And there were other forms such as portal tombs, court tombs and Orcadian round cairns with simple chambers which appear to have no relation at all to the longhouse idea. A desire to emulate and enhance natural forms and materials may be behind the emergence of some megaliths, in Brittany and elsewhere (Scarre 2011). One might predict that a more detailed overall narrative will splinter earlier notions of single origins and of a coherent subsequent dispersal into a kaleidoscope of regional and local sequences, as people sought variously to create new frameworks for their relationships to the land, the past and their dead.

That takes us on to regional sequences. People built many megaliths and barrows, and they did so, for the most part, neither following rigid templates nor creating wholly unique effects; there are recurrent types, more tightly or more loosely definable, and there were changes

through time. There have been innumerable regional schemes of classification for the varying styles of megalith and barrow architecture; opinion here largely diverges on whether to emphasise what is held in common or whether to accentuate the individuality of each construction, with diversity as the main motif. The big, unresolved question is the nature of change through time; whereas the classification of pots seems successfully to yield chronological sequence, this is more complicated in the case of monuments. There are some obvious major trends in some regions. The *allées sépulcrales* of northern France and elsewhere, for example, are not an early form, and the small passage graves of southern Scandinavia, from their associated MN Ia/b material, must also go late in the regional sequence (Ebbesen 2007; Schulz Paulsson 2010; Eriksen and Andersen 2014, 52; Sjögren 2015), both part of a diversity of later and late fourth millennium cal BC architecture.

But how to arrange the varied styles in other cases? In southern Britain there are three major variants of so-called Cotswold-Severn barrow architecture, defined by the layout and positioning of chambers: terminal, lateral and transepted (Darvill 2004). Do these represent contemporary variations on a theme, or a succession of main types? In Orkney, at least three major kinds of tomb can be observed, from modest cairns with simple chambers, to stalled chambers in more elongated cairns, to the Maeshowe type of cairn with passage and more elaborate chambers (Davidson and Henshall 1989). Do these also represent a neat succession of fashions? A recent typology of Irish passage tombs (Hensey 2015) proposes three main types, I–III, of increasing architectural complexity and size, with shifting preferences in location; type I are simple in form, and often coastal, type II more elaborate and often in prominent locations, more inland, while type III include the biggest constructions with both elaborate internal layouts and marked elements of public display, including decoration. On the north European plain and into southern Scandinavia, there are many forms, including earthen long barrows over wooden mortuary structures and graves, stone-defined round mounds with simple megalithic chambers and stone-defined long mounds with simple megalithic chambers (Ebbesen 2007; Eriksen and Andersen 2014; Feeser and Furholt 2014; Müller and Peterson 2015). Is there sequence here?

It would be unwise to neglect the evidence of associated material and stratigraphy. In the Cotswold-Severn distribution, it was noted that three lateral chambered long cairns or barrows overlay traces of later Mesolithic occupation, at Hazleton, Ascott-under-Wychwood and

Gwernvale, and on the grounds of alleged continuity, though against an uncertain timescale, it was proposed that these could be candidates for early constructions. Simple forms of tomb in Orkney do not appear to have material including pottery of later, flat-based style (Davidson and Henshall 1989). The major construction at Knowth, site 1, is preceded stratigraphically by at least two smaller ‘satellite’ tombs (13 and 16: Eogan 1986; Schulting *et al.* in prep.).

So these kinds of scheme should be taken seriously, but how do they stand up when radiocarbon dating is added to the mix? Not enough dating in general has been achieved, for all the prominence of megalithic research in Neolithic studies, and there are the depressingly familiar limitations on sample quality and association. An earlier study of earthen barrows in Britain concluded that they could simply be shown to belong to the fourth millennium cal BC (Kinnes 1992, 120). The sample of well-dated and formally modelled sites remains tiny (Bayliss and Whittle 2007). It is worth noting that the lateral-chambered Hazleton and Ascott-under-Wychwood do appear to begin earlier than the architecturally more elaborate transepted-chambered West Kennet (Bayliss and Whittle 2007), but this represents the grand total of three examples, and no terminal-chambered monument has yet been dated to the same standard. A further twist comes with the dating of Wayland’s Smithy II, very close in style, size and layout to West Kennet, but probably two or more centuries later, in the 35th to 34th centuries cal BC; this was probably at a time of considerable diversity of monuments of all kinds (Whittle *et al.* 2011, 909), and it appears that the builders of Wayland’s Smithy II may have chosen a deliberately old style of construction – for all manner of possible motives, but perhaps including the legitimization of apparent antiquity.

In Orkney, none of the simple forms of tomb, such as those classed as tripartite-chambered and Bookan types (Davidson and Henshall 1989), have so far been dated (Griffiths 2016), so their claimed early position in the sequence remains uncertain, as does their absolute date (Fig. 4.18). How soon after the establishment of Neolithic settlement in Orkney, perhaps in the 37th century cal BC (Griffiths 2016, 296), were the first monuments of this kind built, and how quickly did others follow? In our ToTL modelling of existing dates for Orkney tombs, and following earlier modelling of Quanterness (Schulting *et al.* 2010), it does appear that the Maeshowe type is not early. In itself, that is no surprise, given that there are strong resemblances to the developed passage tombs of eastern Ireland (Sheridan 2004a). What was unexpected was the formal date estimate that both the Maeshowe

and the stalled cairns probably began to be constructed in the 35th or 34th centuries cal BC – in the decades around 3400 cal BC; it is not possible to tell which came first (Bayliss *et al.* 2017) (see again Fig. 4.6). That probably makes the Maeshowe type earlier than the great exemplars of eastern Ireland, though Maeshowe itself remains frustratingly more or less undated, except by less than perfect samples from its ditch (Bayliss *et al.* 2017). On this model, stalled cairns have to be seen as creating an alternative rather than an earlier set of ritual and mortuary practices. The date estimate for the appearance of both kinds might also imply a burst of activity a couple of centuries or more after the establishment of Neolithic settlement on the islands, not unlike the pattern suggested for southern Britain (Whittle *et al.* 2011).



Fig. 4.18: Details of stonework in Orkney tombs. Clockwise from top left: Midhowe; then Maeshowe. Photos: Colin Richards and Antonia Thomas.

In Ireland, it has been argued that it is a moot point whether the monuments at Carrowmore are to be classified rigidly as passage tombs (Cummings and Richards 2016), and in any case the previously available dates on charcoal did not suggest anything there earlier than c. 3500 cal BC or later (Whittle *et al.* 2011, fig. 12.45; see Bergh and Hensey 2013 for a slightly less cautious view). Cremated bone pins from the monuments at Carrowmore have recently been dated to c. 3600–3200 cal BC (Bergh and Hensey 2013), and the special pleading that previous contents could have been completely cleaned out, thus allowing a very early dating compatible with passage grave development in Brittany in the later fifth millennium cal BC (Sheridan 2003a; 2003b), is highly unconvincing. By the same token, it is also unlikely that various constructions on the west side of Britain can all be related to Breton passage graves and thus claimed as early (Sheridan 2004b); not only is the classification here suspect, but the dated dolmen at Garn Turne in south-west Wales (Fig. 4.19) appears to be probably no earlier than the 38th or 37th centuries cal BC (Cummings and Richards 2016, 56). With much greater confidence, both Newgrange and Knowth 1 belong late in the fourth millennium cal BC, so that a late flourish of large constructions with prominent public display is certainly plausible (Hensey 2014), but it is to be noted that simpler passage tombs, as at the Mound of the Hostages (Whittle *et al.* 2011), are also of the same kind of date.



Fig. 4.19: The portal dolmen at Garn Turne, Pembrokeshire. Photo: Vicki Cummings.

Surprises came too from the formal modelling of the megalithic monument at Flintbek LA 3 in northernmost Germany, close to Kiel and the sea (Mischka 2011) (Fig. 4.20). This contains a variety and evident succession of graves, megalithic chambers and stone settings or ‘frames’ defining mounds. In very brief outline, a succession of graves under small mounds began to form a long narrow mound, to be succeeded by two simple chambers or dolmens, probably earth-covered, and themselves framed by a rectangular setting of small stones, and then by another grave and a third dolmen, with the whole mound now framed by a larger rectangular megalithic setting; finally the barrow was doubled in size, with the kerb now extended, but the new space occupied only by a fourth dolmen, only slightly larger in size than its predecessors (Mischka 2011, 745–6). Previous estimates had proposed nonmegalithic barrows in the 38th–37th centuries cal BC, followed by megalithic chambers in stone-lined long barrows in the 36th–35th centuries cal BC, but at Flintbek the 11 phases of activity were over in about a century, principally in the 35th century cal BC (Mischka 2011, 755). This need not invalidate the previous longer chronology, and the evidence of associated pottery and axes is again relevant, but it may again strongly suggest the possibility of more accelerated sequences. Classifying monument types on its own is

an unreliable guide to order and timing. As Doris Mischka notes (2011, 743), finds are rarely in contexts that define construction and there has been a lot of clearing out and disturbance. In the case of the Orkneys, there was also a period in the later fourth millennium cal BC when round-based and flat-based pot styles overlapped, so the simple presence or absence of one or the other is again an unreliable guide to date. As with the beginnings of the phenomenon as a whole, more dating and more modelling are urgently required everywhere to establish better regional sequences.

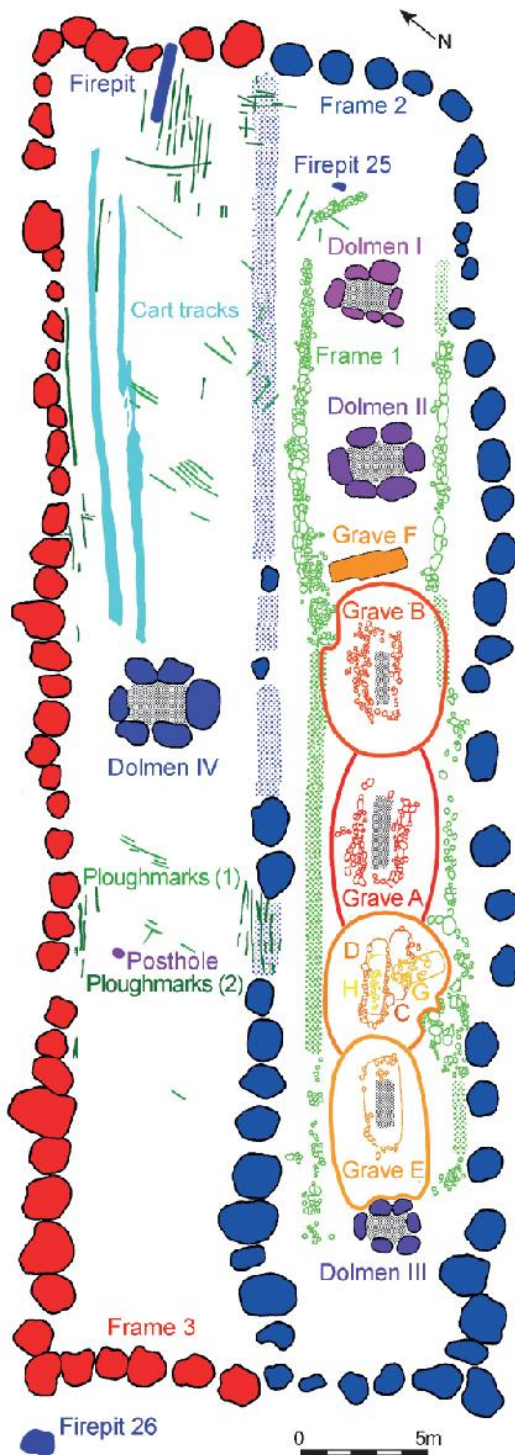


Fig. 4.20: Plan of the principal features in the Flintbek LA 3 monument. By kind permission of Doris Mischka (see Mischka 2011).

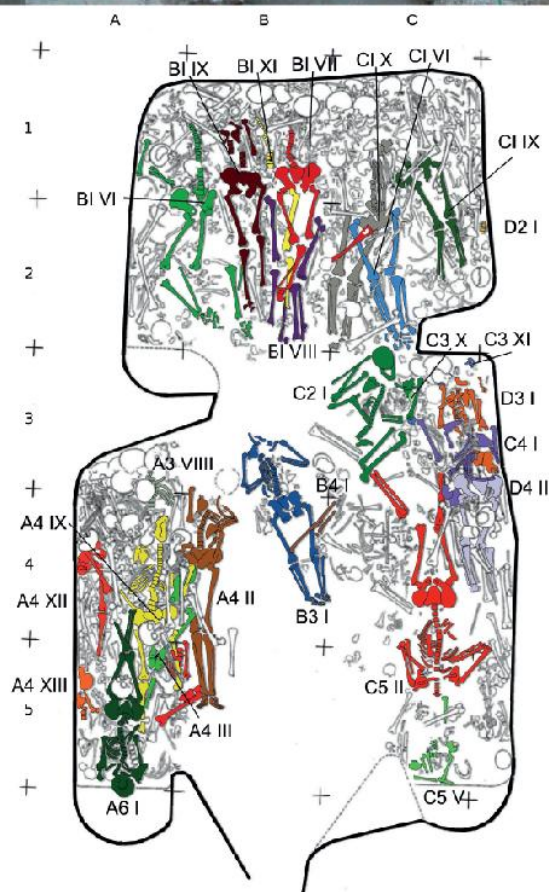


Fig. 4.21: Top: deposits of human bone in part of the allée sépulcrale at Bury (photo: Laure Salanova); below: plan of the deposits in the hypogée at Les Mournouards (after Blin 2015;

We may be on securer ground when it comes to estimates of the duration of activity in these kinds of monuments, though again the sample of well investigated sites is still pitifully small. A number of studies suggest shorter rather than longer spans of activity. Flintbek, just discussed, is a case in point with a century or so of activity represented (Mischka 2011), and within these four generations seems to be contained a rapid process of increasing monumentalisation. In our study of southern British long barrows, Ascott-under-Wychwood stood out as being in primary use – for the deposition of just over 20 people – for probably three to five generations, whereas Hazleton, West Kennet and Fussell’s Lodge – probably with not much more than 40 people represented in each case – were probably only in primary use for one to two generations, and deposition of the first phase of Wayland’s Smithy is compatible with something even more swift, possibly even event-like (Bayliss and Whittle 2007).

Larger numbers of people were deposited in the *allées sépulcrales* of the Paris basin, many of them as visibly more or less intact bodies, mostly lain extended (Chambon 2003; Blin 2015). From the associated material and a scatter of previous radiocarbon dates, these can be assigned to the *Néolithique récent* of the second half of the fourth millennium cal BC, with some activity extending into the *Néolithique final* of the earlier third (Salanova et al. 2011). Simply from the numbers of people involved, often reaching into the hundreds, it could be supposed that these were communal ossuaries in use for many generations. In the ToTL project, we helped to date two striking examples: the 20-m long *allée sépulcrale* at Bury, Oise (Salanova et al. 2017; Healy et al. in press), and the underground, chalk-cut *hypogée* at Les Mournouards, Marne (Leroi-Gourhan et al. 1962; Chambon et al. 2018). Bury probably contained nearly 300 people, though belonging to two phases of use (Salanova et al. 2017, 61), while Les Mournouards held the remains of 79 people (Fig. 4.21). While distinctions can be made in terms of location within the elongated chamber of Bury, there was no further architectural demarcation within it, whereas there are two chambers within Les Mournouards, and from the beginning the excavator argued for two social groupings of some kind. Here at least there was no shortage of samples, though we had to steer round the enthusiastic use in the 1960s of consolidant for some of the remains. There was also meticulous recording of both deposits, and on the basis of that for Les Mournouards, a case was made for two phases of activity, a first one represented in the final accumulation only by disarticulated remains. The probable process of

successive depositions is seen in Les Mournouards not only by some rearrangement of skulls, but also by the vivid example of what looks like the last deposit, that of a pregnant woman, placed in the centre of the main chamber.

The preferred models for both tombs suggest shorter rather than longer spans of use: for Bury, perhaps for as little as 60 years (Healy *et al.* in press), and for Les Mournouards probably for as little as 100 years, with both its chambers in concurrent use (Chambon *et al.* 2018). The duration of deposits and monument use is not reliably to be derived simply by looking at numbers of people and the scale of the architecture. Those propositions conceal a lot of modelling effort; the Bayesian process is not always straightforward, and the reader can go to the detailed reports for the gory detail. On the one hand, there is a lot of constraint in the stratigraphic matrix for Bury, with long strings of relationships among the successively deposited dead; that should be a help to precision. On the other hand, those are crowded into an exceptionally unfriendly area of the calibration curve in the 33rd and 32nd centuries cal BC. For the more dispersed dates and shorter stratigraphic strings of the deposits of the *Néolithique final*, the impact of slighter plateaux in the 28th, 27th and 26th centuries cal BC is less severe. In both phases, the distributions of individual dates, even when constrained by the models, are extended and sometimes bimodal, so that the resulting estimates are less precise than they would be had the dates fallen elsewhere on the curve.

Other probable examples of short use can be found among exceptionally large monuments. The main mound at Knowth and its ‘satellites’ represent the best dated complex among the largest Irish passage tombs. One model in a recent programme of dating, including of cremated human bone, and modelling places it between the 32nd and 30th centuries cal BC, with a span of use of *110–170 years (68% probability)* (Schulting *et al.* in prep., model 5, table 4.10). That encompasses both the initial tomb 1B, with its two chambers, and its enlargement as tomb 1C.

With a diameter of 75 m, the mound covering the tholos at Montelirio within the complex at Valencina de la Concepción (Fernández Flores *et al.* 2016; García Sanjuán *et al.* 2018) is not markedly smaller in diameter than that of Knowth tomb 1, though much lower, and it appears to represent a single build. It takes its place among an unusual concentration of monuments, ‘artificial caves’ and pits with mortuary deposits, pits, major ditches and other features, spread over more than 400 ha (García Sanjuán *et al.* 2013a). Whether this was just a place for periodic gathering and mortuary

ritual, or some kind of settlement focus as well, it represents a much greater concentration of activity than seen earlier in the regional sequence of this part of south-west Spain. This begins at Valencina in the late fourth millennium cal BC, and the earliest mortuary activity is represented by the start of successive, simple inhumations in an artificial cave at La Huera. This is probably the kind of date when the novel architecture of tholos tombs begins across southern Iberia (Balsero Nieto *et al.* 2015). At Valencina itself, it is possible – though the sample of dated sites is small – that tholos tombs were used principally in a relatively short horizon in the earlier third millennium cal BC (García Sanjuán *et al.* 2018, figs 54–5) (Figs 4.22–4.23). The Montelirio tholos itself has a characteristically circular main chamber, with a smaller secondary chamber, their stone slate walls painted bright red. It is approached by a 35-m long passage. The main chamber, partially disturbed, contained the articulated remains of at least 20 adults (see Figs 1.5–1.6 and 5.13), and the secondary chamber, also disturbed, the remains of at least two. Both chambers had striking finds in them and the dead probably had elaborate funerary dress; I will look at these in more detail in Chapter 5. The condition of the human remains suggests that the formation of the deposit could have been rapid (Fernández Flores and Aycart Luengo 2013; Fernández Flores *et al.* 2016). In this context, a different kind of challenge for radiocarbon dating is presented by the low levels of preserved collagen in the human remains. With the omission of one outlier, the radiocarbon determinations on all seven dated individuals from the main chamber are statistically consistent and these people could have all died at exactly the same time. Two out of a number of alternative models estimate the construction and use of the Montelirio tholos within a restricted period in the later 29th or 28th century cal BC. The radiocarbon dates are compatible with the interpretation of burial in the main chamber as a single event, but do not prove that this was the case. The radiocarbon evidence is also compatible with these burials occurring over a period of a few decades. In either case, the use of this spectacular tholos appears to have been shorter rather than longer.

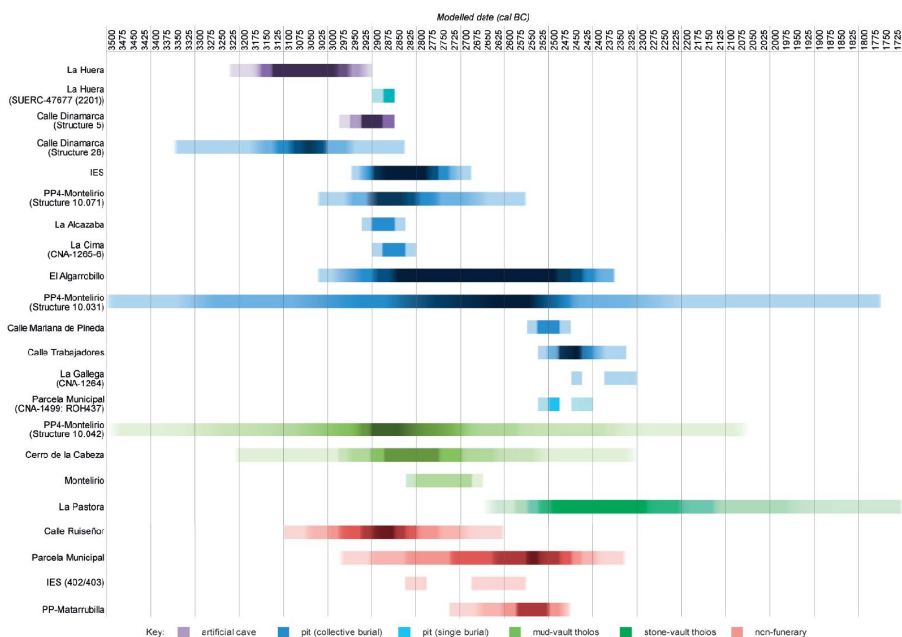


Fig. 4.22: Model for the currency of the principal forms of burial and mortuary architecture at Valencia de la Concepción. After García Sanjuán *et al.* (2018).

Finally, it is important to note that short spans of time may not typify the use of all such monuments. Long, probably episodic use of a tholos at El Barranquete in south-east Spain has been formally modelled (Aranda Jiménez and Lozano Medina 2014), and episodic use over long spans of time has also been seen at Coldrum and Poultnabrone (Wysocki *et al.* 2013; Lynch 2014). The numerous remains in the Maeshowe passage grave at Quanterness in Orkney could have been deposited over a long period, from c. 3400 cal BC to c. 2800 cal BC (Schulting *et al.* 2010; Bayliss *et al.* 2017, fig. S6), though some researchers see some of the human bone as being derived from older deposits elsewhere, which would shorten this span. We are therefore going to have to get at durations on a case by case basis.

Enclosures

I have already sketched in [Chapter 1](#) what we learnt from an extensive study of causewayed enclosures in southern Britain. We could time their first appearance, probably from the late 38th century cal BC, and could track both their spread, probably from east to west across southern Britain, and the acceleration in numbers through the 37th century cal BC, followed by an apparent dip and then the final round of fresh construction in the first part of the 36th century cal BC; we were able to distinguish between the spans of the primary use of

enclosures, both longer and shorter, some probably less than a generation and even event-like, and we could use the timescales established to quantify the deposition that is so characteristic of the ditch fills. For all this, we used some 400 existing dates and obtained another 400 or so new ones, and by gathering up more than a further 1400 existing dates for other early Neolithic contexts, not just in southern Britain but in Ireland and Scotland south of the Great Glen, we were able to propose a provisional overall narrative for the development of the Neolithic in Britain and Ireland, in which causewayed enclosures came in not right at the beginning but after some three centuries (Whittle *et al.* 2011). This surely all demonstrates what could be achieved in the many other parts of western Europe where there are broadly comparable horizons of causewayed enclosures or interrupted ditch systems: imagine what a detailed narrative of enclosure building and use could be like, in the wide sweep from southern Iberia to northern Germany and southern Scandinavia, and how that might be integrated with a parallel detailed narrative for megalith construction over the same broad area!

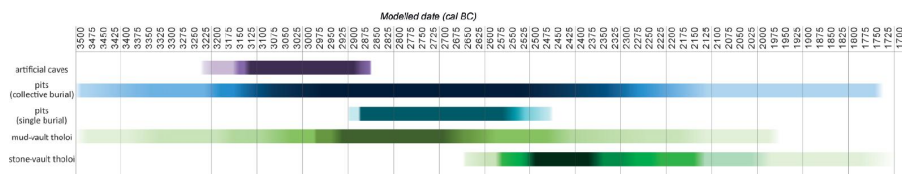


Fig. 4.23: Model for the diversity of the principal forms of burial and mortuary architecture at Valencina de la Concepción. After García Sanjuán *et al.* (2018).

Despite these gains in understanding for southern Britain, there are plenty of remaining puzzles. Prominent among them is that although it looks as though there is some kind of coherent sequence over the first centuries of the Neolithic in southern Britain in which monument building was at first absent or extremely scarce, followed by the first regular appearance of long cairns and long barrows, and then a century or less later by the enclosures themselves, we cannot yet grasp in detail the specific conditions in the late 38th century cal BC which encouraged or enabled the adoption of what were dramatically new practices, in both kind and scale (Whittle 2013). We were at least able to suggest an extension of the geographical range of exchange networks in the 37th century cal BC, part of a surge of innovations and changes from the later 38th century cal BC, which may be an important clue to the circumstance in which enclosures themselves were introduced (Whittle *et al.* 2011, figs 14.134 and 14.143), but the settlement record in southern Britain is simply not refined enough to let us track changes in the landscape, though more sophisticated,

Bayesian modelling of pollen sequences does hold out the promise of offering a partial, proxy view, at least in areas where pollen survives (Farrell *et al.* submitted). Did enclosure construction and use, with its demands on labour and the need for audiences and participants, only become possible when a certain level of population density was reached? Did enclosures reflect conditions of competition and the need for both self-definition and neutral places for negotiation and interaction among dispersed communities? Or, given the undeniable precedents in similar enclosure construction in adjacent continental Europe, was there something about conditions there in the 38th century cal BC which encouraged the adoption of new practices in southern Britain in the late 38th century cal BC?

So in this section, I want, as with megaliths, briefly to review issues of the timing, order and duration of enclosures comparable to those in southern Britain, in order to assess the prospects for the same kind of detailed narrative as has been shown in southern Britain to be achievable. I am going to concentrate therefore on what appears to be a broadly comparable set of practices of enclosure construction and use across western Europe, using ditch circuits that are frequently causewayed or interrupted, from the later fifth well into the fourth millennium cal BC.

We know that enclosure construction goes back to the LBK, and that early examples of interrupted ditches are to be found in late LBK and post-LBK contexts, for example in the Paris basin (summarised in Whittle *et al.* 2011; Whittle 2014). We also know of other kinds of ditched enclosures, such as the fifth-millennium *rondels* of central Europe, and other kinds of ditches were used from an early date to define settlements and tells in contexts from southern Italy to the Balkans and Greece (Andersen 1997). So there is not perhaps the same kind of question about beginnings or origins as faces the study of megaliths, but in terms of the development of distinctive enclosures in the post-Danubian world from the later fifth millennium cal BC, there are still important, unresolved questions of timing.

At the moment, it looks likely that some of the first enclosures of this kind to be constructed and used in any numbers belonged to the Michelsberg culture (Meyer and Raetzl-Fabian 2006; Seidel 2008; Geschwinde and Raetzl-Fabian 2009; Geschwinde 2013; a more detailed account is given in Whittle *et al.* 2011, 878–85). Using the conventional periodisation of the Michelsberg culture (going back to Lüning 1967), there are enclosures with MK I pottery at Bazoches and other sites in the Paris basin (Duboulouz 1998; Lietar 2016), Koslar, Mayen, Miel, Urmitz (Meyer and Raetzl-Fabian 2006, 41–2) and

Ilsfeld (Seidel *et al.* 2016, Tab. 8). The currently widely accepted view is that the Michelsberg culture first emerged in northern France and spread eastwards (Jeunesse *et al.* 2004). Our ToTL study of the cultural sequence in Lower Alsace modelled the date of the end of the middle Neolithic in the very late fourth millennium cal BC, and regionally the late Neolithic there begins with MK II assemblages (Denaire *et al.* 2017); that is certainly compatible with the notion of an earlier emergence of MK I to the west. But as already noted, there are divergent views on how early MK I begins, at either c. 4500 cal BC in a high chronology (Jeunesse *et al.* 2004) or later, perhaps c. 4300 cal BC; neither has yet been established by extensive radiocarbon dating or formal modelling. This matters. Not least, how do the sequences of megalith construction in north-west France and of enclosure construction in northern France and the Rhineland run relative to each other? Do they somehow run in parallel? Enclosures are conspicuous by their absence in Brittany, and megaliths likewise at this horizon in northern France and the Rhineland.

By the horizon of MK II, more enclosures are known in the Rhineland (Seidel 2008; Seidel *et al.* 2016, Tab. 8), and subsequently through the Michelsberg sequence there seems to be a process of ‘Ostdrift’, with more enclosures appearing to the east as time goes on (Müller 2011a; Geschwinde 2013). The first phase of the enclosure at Büdelsdorf in northernmost Germany, with associated FNI TRB pottery, has been modelled as belonging between the mid-38th and mid-37th centuries cal BC (Hage 2016). We do not yet have a detailed chronology for enclosure development in northern France (some with northern Chasséen associations), partly because it has proved impossible to radiocarbon date the many enclosures on river terraces, such as Bazoches, in a tributary of the Aisne, due to poor collagen preservation in potential bone samples (Jérôme Dubouloz, *pers. comm.*). Nonetheless, there remain tempting targets for the future, such as the intriguing sequence of depositions, including articulated animals, perhaps some kind of herd-scale slaughter, through the fills of the chalk-cut ditch of Boury-en-Vexin, Oise (Meniel 1987).

There are abundant enclosures in central-west France, with regional variations on the overall theme, in terms of distinctive features of ditch and entrance layouts. We do not yet have a detailed chronology for these, though it seems unlikely that they predate the first Michelsberg enclosures, and may belong to various points in the fourth millennium cal BC, perhaps to its first half (Cassen and Scarre 1997; Burnez *et al.* 2001). So we cannot tell if these coincide with, precede or follow the appearance of enclosures in southern Britain.

Previously, the enclosures of southern Scandinavia have been assigned to the ENC–MNA1a–b horizon, with an estimated span c. 3500–3200 cal BC (Andersen 1997; Nielsen 2004); more recently, the date of the enclosure at Liselund in north-west Jutland has been formally modelled as belonging to the 37th century cal BC (Torfing 2016; cf. Klassen 2014), and others from southern Jutland as well as northern Germany are suggested to be of comparable date (Torfing 2016, 12; see again Hage 2016 for Büdelsdorf).

Southern Chasséen enclosures in southern France are thought to have been in use from the later fifth millennium cal BC (Demoule *et al.* 2007; Vaquer 2011). Large, complex examples in the upper Garonne valley include St-Michel-du-Touch and Villeneuve-Tolosane/Cugnaux (Gandelin 2011); northern influence in their appearance is suspected (Vaquer 2011, 242), and without a regional tradition of enclosures preceding these, unlike in the Paris basin, that is for the present plausible. Both those two large enclosures were dug over a long period and it has not proved easy to radiocarbon date them. In the ToTL project, we helped to date the newly excavated double-ditched enclosure at Château-Percin, just outside Toulouse (Gandelin *et al.* in press) (Fig. 4.24), but were able to provide an outline, provisional model for Villeneuve-Tolosane/Cugnaux as well. That enclosure began with palisades, but was later defined by two successive ditch systems, each enlarging the circuit. With appropriate caution about the available dates and samples, the model suggests a construction date for the first ditch c. 4200 cal BC and for the second c. 4000 cal BC (full details in Gandelin *et al.* in press); other Chasséen activity on the site, including fired stone settings (the enigmatic ‘structures à galets chauffés’), pits and burials, probably began earlier, perhaps by up to a century (Gandelin *et al.* in press, fig. 21). The same pattern is evident in the more robust but not unproblematic results for Château-Percin, since three valid models can be constructed. Our preferred model (1A) follows the interpretation that the site represents a continuous phase of activity, and that the interior ditch preceded the exterior one. There were again striking numbers of ‘structures à galets chauffés’, in both round and rectangular settings. Their use, with Chasséen ceramic associations, appears to have gone back to the 44th or earlier part of the 43rd centuries cal BC, whereas the inner ditch and rampart were constructed and then quickly and extensively burnt c. 3900 cal BC, to be followed shortly by the outer ditch in the first part of the 39th century cal BC (Gandelin *et al.* in press, figs 10–12). These models give useful insights into the possible character of enclosure at sites long in use for some kind of gathering and perhaps

public feasting, and potentially inform on a rapid spread of new practices across parts of France, but much more precision for both north and south remains to be established by future research.



Fig. 4.24: The enclosure and other features at Château-Percin. Top: structures à galets chauffés ; middle: the two ditches under excavation; lower: burning in the inner ditch. Photos: S. Puech/Inrap.

Finally, new research over the last decade or more has established the presence of many ditched enclosures across several parts of Iberia, notably in the south-west, where the most intensive survey and excavation, at spectacular sites like Perdigões, have been carried out (Valera 2016; Jiménez-Jáimez and Márquez-Romero 2016). In the south-west, enclosures appear on current evidence to emerge in the last three centuries of the fourth millennium cal BC, though unpublished dates from the Alentejo may go back to c. 3500 cal BC

(Valera 2016, 70). This may mirror the apparent late dating for the appearance of enclosures in southern Scandinavia.

If there is still much uncertainty in this wider narrative for the appearance of enclosures across western Europe, there is also no clear pattern for subsequent development. Enclosures do not offer themselves to the same kind of typological classification as megaliths and related monuments. In the middle Rhineland and the Neckar valley, enclosures of various sizes and with varying numbers of circuits can be found through the Michelsberg sequence (Seidel 2008; Seidel *et al.* 2016, Tab. 8). There were already substantial enclosures such as Bruchsal 'Aue' in MK II, and others followed in MK III and MK IV. The largest Michelsberg enclosure by far, Urmitz, which bounds some 100 ha, could have developed over several MK phases, but most if not all of the site appears now to have been destroyed by urban development and no radiocarbon dating has yet been possible (Boelicke 1978, figs 28–33; Gronenborn 2007b); radiocarbon dates for the ditch and rampart at Kapellenberg, near the confluence of the Main and Rhine, are also awaited (Gronenborn 2010), and the 60-ha enclosure at Wiesbaden-Schierstein, just below the confluence, is also insecurely dated (Fig. 4.25). A trend has been claimed from larger and more segmented enclosures in the early MK, some located in river valleys or on low hills, to later MK enclosures often found in topographically prominent and potentially defensive situations (Matuschik 1991; Seidel 2008; Geschwinde 2013). Michelsberg itself, probably belonging to MK IV, located on a hill and covering more than 10 ha, may, however, suggest a less linear development. There is much to sort out.

In the ToTL project, we obtained more dates and formally modelled the chronology of the small, double-ditched enclosure of Klingenberg 'Schlossberg' in the Neckar valley (Seidel *et al.* 2016) (Fig. 4.26). This dates to the late MK V/Munzingen phase. It appears to be part of a late phase of enclosure construction, with four or five sites in the region representing new foundations, in contrast to sites such as Bruchsal 'Aue' which had gone through a series of modifications since MK II. The Klingenberg promontory had been used for occupation in MK II and MK III/IV, but the enclosure can be modelled as being started c. 3800 cal BC, probably with the construction of the outer ditch, followed very shortly by the inner ditch, and soon after again and perhaps all within a single generation, by the use of pits in the interior and then outside the circuit. The enclosure appear to have ended in the 37th century cal BC, with intense burning seen in both ditches and including remains of the single rampart thought to have

stood between them; a few pits in what had been the interior may have continued in use for a short period after this event. There is a puzzle, however, because the modelled estimates suggest a duration of more than a century whereas the archaeology of the ditch fills gave the impression of rather more rapid events following initial construction; partial recutting of the first ditch fills might be one way to resolve this. It is also not possible to determine whether the apparently event-like ending of the enclosure was due to hostile attack by outsiders or ritualised demolition by the owners of the site (Seidel *et al.* 2016). This study therefore does not give complete answers, but it does indicate what could be achieved in future research, using the archives of previous investigations as well as samples provided by new excavation, to refine the precision of currently available chronology.

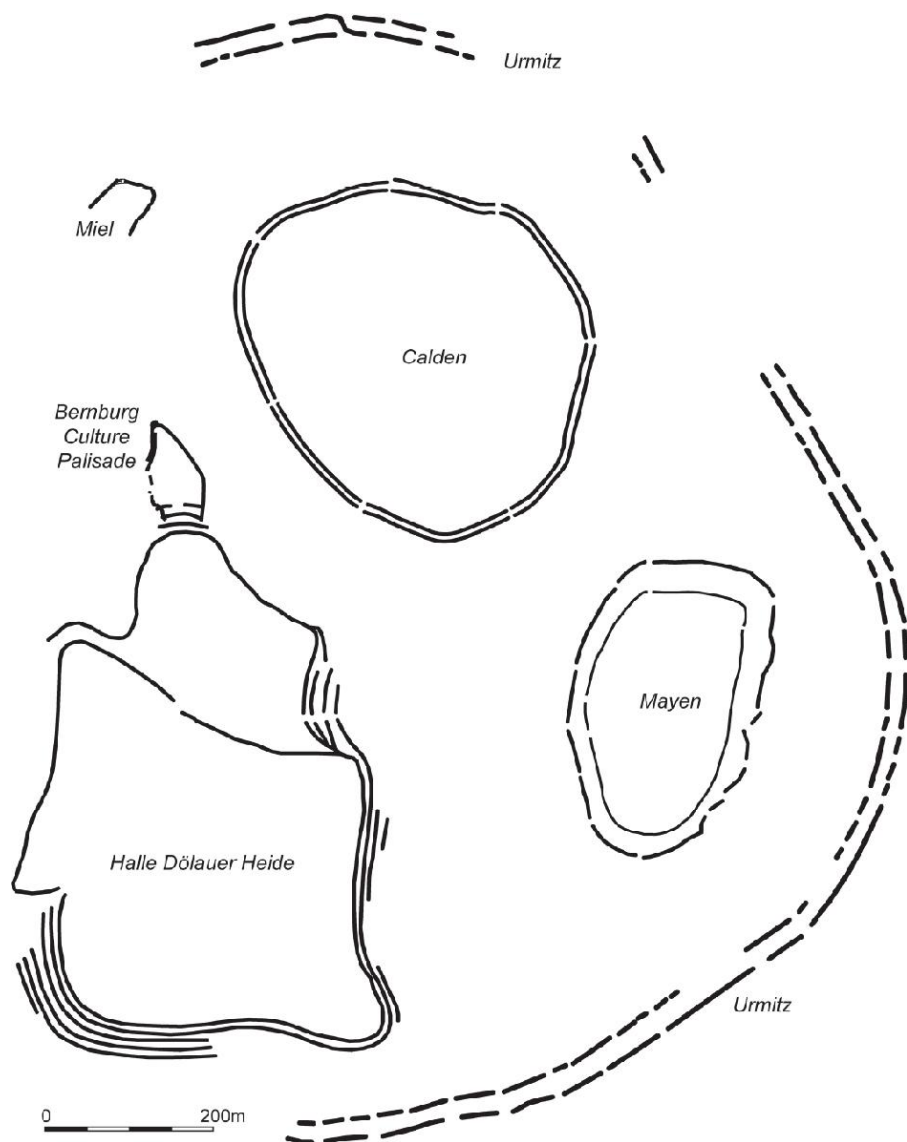


Fig. 4.25: Plans of selected Michelsberg and other enclosures. After Whittle et al. (2011).



Fig. 4.26: The late Michelsberg enclosure at Klingenberg 'Schlossberg'. Clockwise from top left: aerial view of the double circuit; ditch excavation; burnt timbers in the inner ditch; burnt features in the fill of the inner ditch, feature 169. Photos by kind permission of Ute Seidel and Landesamt für Denkmalpflege Baden-Württemberg; © Landesamt für Denkmalpflege im Regierungspräsidium Stuttgart.

Intriguing patterns have been suggested for the development of enclosures in the Paris basin, after a date informally estimated as 4250 cal BC, in what is defined regionally as the second phase of the middle Neolithic (Lietar 2016). These follow earlier ones in the first phase of the middle Neolithic, as noted above. Four kinds of enclosure have been suggested on the basis of multivariate analysis: 'uncomplicated', generally simple circuits, 'lacking evidence for habitation' and with rare complex deposits, but with considerable variation in size (D); small sites with a claimed domestic function (C); complex sites, of varied layout and size, with evidence for occupation and for complex deposits (B); and 'monumental' or 'ceremonial' sites, of varied layout, generally on a large scale, with no evidence for habitation but with 'relatively complex deposits', such as cattle skulls, faunal remains, antler and pottery (Lietar 2016, 33–4). One could quibble with this classification, on the basis of normally incomplete excavation and the lack of precise date estimates for the duration of primary use, but

taken at face value, a pattern is suggested of simple C enclosures, such as Balloy, in the preceding Cerny phase in the middle of the fifth millennium cal BC, followed by a marked increase in diversity after 4300/4250 cal BC, with examples of large, complex and small (A, B and C). Monumental enclosures appear first in this scheme after 4250 cal BC in those areas with Michelsberg pottery, but only after a claimed date of 4000 cal BC in the area of the Noyen group, centred on the area of the Yonne-Seine confluence or Bassée. What is called a final phase of enclosure construction begins around 4000 cal BC, with a high proportion of the simple D enclosures, particularly in areas characterised by the use of Chasséen pottery, in the west of the Paris basin (Lietar 2016, 34). This indicates the potential for further differentiation, but it is worth remembering that the dates in question are informal estimates, and the spans of time used are very broad. It is not yet possible to get a detailed sense of the tempo of change. The first complex B enclosures are ascribed to the post-Rössen phase of the Aisne valley, at 'c. 4300 BC' (Lietar 2016, 34). Was there a gradual beginning to more diverse enclosure construction in the 43rd century cal BC, or did things take off rapidly, and what was the tempo subsequently?

The lack of formal date estimates also precludes precise estimates of duration. Those for Château-Percin and Klingenberg have been noted above. Both were probably in use for relatively short periods, but it was not possible in either case to demonstrate the very short spans seen at some of the southern British causewayed enclosures. The enclosure at Bazoches (Dubouloz 1998), monumental or ceremonial in Lietar's classification, has at least four phases of development, but its pottery appears confined to MK I, 'between 4250 and 4050' cal BC (Lietar 2016, 37), with the implication, if such informal estimates are not importantly wrong, that each site phase might also have been of shorter rather than longer duration. Potentially longer histories of enclosure use, as noted above for Michelsberg sites like Urmitz and Bruchsal 'Aue', could well be constituted by a series of individually shorter episodes, rather like the biography of Hambledon Hill in southern England (Mercer and Healy 2008; Whittle *et al.* 2011).

Once again, there is much to do, and this matters. It matters because we cannot yet fully put these sites into their full context. We cannot tease out all the intricacies of the sequence across the complex cultural geography of the Paris basin at the start of the second phase of the middle Neolithic, nor follow in precise detail what looks like the wider expansion of enclosure construction and use into other areas, including west-central France, southern France and ultimately

Iberia, east of the Rhine and into northern Germany and southern Scandinavia. We cannot provide a wide freeze-frame for the later 38th century cal BC, when the first enclosures were appearing in southern Britain. Nor can we track in detail the parallel and potentially inter-related or complementary histories of megalith and enclosure construction across northern France as a whole. In the case of the great range of settlements and houses described above, there is perhaps fundamentally no shortage of suitable samples to be dated, including in the archives of old excavations; the challenge is to mobilise projects and funding to get many more neighbours precisely dated. With the two kinds of monument discussed in this section, the task is likely to be much harder, since suitable dating material is scarce in many contexts. That does not, however, necessarily apply everywhere, and perhaps more precise dating will have to rely on selected examples where the conditions of survival are more favourable. The other way round the obstacles may be to refine the typologies for material sequences, including especially of pottery, and to date ceramic assemblages at as many Michelsberg sites as possible with favourable conditions for sample survival, for example, or from the direct dating of fatty acids in the pottery itself (see [Chapter 7](#)), such that the chronology of particular enclosures could be extrapolated from a more precisely measured material sequence. On that hopeful note, I turn now to things in their own right.

The times of things

Douglass Bailey once referred to the ‘possession revolution’, to describe the great increase in things through the millennia of the Neolithic and Copper Age sequence in south-east Europe.⁵ In many ways, this also evokes the general condition of Neolithic existence across the continent as a whole. Things were regularly made in quantity, often with great skill (J. Chapman 2014), producing an array of surfaces, finishes, colours and forms, from a very wide variety of materials. A Neolithic existence without pots, axes, figurines or ornaments is unthinkable. In past research, all these things could sometimes be taken for granted, as the somehow inevitable accompaniment of settled life or predestined expression of group identities and increasing social differentiation. For some scholars, it has been important to emphasise the appearance of metals – copper and gold – and to define a Copper Age as an evolutionary stage of societal development. (How far that is a useful concept is open for debate, and answers will vary from region to region (e.g. Bartelheim and Krauss 2012; Schier 2014); it is much more profitable in my view, as should be clear by now, to define the passage of time as precisely as

possible, rather than to start with predefined epochs or ages.) Generalising, most approaches have seen material things as the product of human agency, variously used to project group or individual identities and social relationships and to communicate a plethora of symbolic meanings; the emphasis given to such themes obviously varies across the spectrum of past interpretive stances, from culture history to processualism to post-processualism. More recently, a radically different view of the possibilities of things has emerged, in which they are seen variously as potentially having vibrancy, animacy and agency, in the sense of being able to effect changes in the world (Harris and Cipolla 2017, and many references).

Generalising again, all such views of things have tended to operate within fuzzy chronologies. I have already described in [Chapters 2 and 3](#), for example, some of the timescales regularly found in the culture history approach, and noted the seemingly timeless contexts in which other kinds of interpretations of things can be framed. So whatever position we choose to adopt with regard to the roles, meanings and efficacy of things, the first challenge should be to time them better.

In this section I am going to review several ToTL studies of things, principally pottery, in order to emphasise how complex and how diverse their timings and durations were. I will also bring in reflections on material sequences in the Alpine foreland, exploiting further the precise dendrochronologies noted in [Chapter 3](#). This is to advocate the need for many more such detailed studies of material, using the potentially powerful combination of formal chronological modelling and traditional means of ordering the material such as typology and seriation.

I am principally going to look at the development of pottery in three situations already described earlier in this chapter: in the sixth–fifth millennium cal BC Vinča culture of the northern and central Balkans; in Lower Alsace in the upper Rhineland, from the LBK of the later sixth millennium cal BC to the end of the ‘Danubian’ tradition towards the end of the fifth millennium cal BC; and in Orkney from the later fourth to the mid and later third millennium cal BC. I will also briefly bring in studies from the Alpine foreland, informed by precise dendrochronologies. I will then briefly note some of the implications for our view of things, focusing especially on the recent ‘ontological turn’, as one strand in my wider argument about the kind of history to be written for Neolithic lives.

The Vinča complex

Across its broad distribution through the river valleys – the Danube, its tributaries and their catchments – of the northern and central

Balkans (from southernmost Hungary and easternmost Croatia through Serbia down to Kosovo and parts of Macedonia and Bulgaria, and from Croatia and Bosnia-Herzegovina eastwards as far as parts of Transylvania in Romania), the Vinča culture represents a series of important changes in the developing Neolithic way of life in the region. These include the spread, consolidation and diversification of settlement, involving the formation of large settlements and tells; the intensification of subsistence; and the emergence of both larger communities and distinctive households within them – and changing materiality and the expansion of material networks (J. Chapman 1981; 2000; Kaiser and Voytek 1983; Tringham and Krstić 1990; Tripković and Milić 2009; Orton 2010). Big, unresolved questions abound. How did this complex or network come into existence? Was there continuity with what came before, or did change come from the outside? Who or what were the drivers of change within the expanding and established network? And how and why, after a duration of centuries, did it all come to an end in the middle of the fifth millennium cal BC?

This is an archaeological record dominated by the evidence of settlements, in the form of tells and ‘flat’ sites. Within these, houses are prominent, normally closely set, often in rows on either side of narrow lanes or streets, and sometimes within large flat sites, when geophysical survey has been extensive enough (Crnobrnja *et al.* 2009; Perić *et al.* 2016), in what appear to be neighbourhood groups. We know much more about the internal layout and contents of houses later in the sequence; generally speaking the evidence for early houses is much more fragmentary.

Well preserved contexts such as houses at Crkvine-Stubline, part of a local network of sites of varying size, some 40 km southwest of Belgrade and the great tell of Vinča-Belo Brdo (Crnobrnja *et al.* 2009), had lots of pots in them: ‘dozens of ceramic vessels’ and ‘a large number of storage vessels’ in house 1/2008 (Spasić and Živanović 2015, 220). Some of these may have been stacked on wooden shelves (Crnobrnja *et al.* 2009, 19). There are plentiful other examples, such as at Divostin (Tripković 2010). Occasionally pots are found in the spaces outside, in between houses (Nikolić and Vuković 2008). This household setting is therefore presumably the context in which Vinča pottery was principally used, for a wide range of tasks, from food preparation (though the direct evidence for that is surprisingly sparse), storage and consumption. It may be that vessels most open to public gaze, literally passable from hand to hand, such as certain types of bowls, were the ones most likely to change the fastest through time,

as evidenced in the correspondence analyses of bowls from Vinča-Belo Brdo (Schier 1996; 2000); these might also have been the vessels most likely to break (David 1972).

Prodigious quantities of pottery have come out of sites like Vinča-Belo Brdo itself, and there seems no doubt that there were large sets of pottery in active use by people for both the storage and the presentation of food, part of wider sets of household equipment and fittings including tables, hearths and storage facilities, which help to define a suite of eating practices (Spasić and Živanović 2015). In the long-running and understandable concern with typological classification and sequence, the wide range of shapes and sizes for Vinča pottery should not be overlooked (Tasić *et al.* 2016a, fig. 10; see again Fig. 3.6). At the smaller end, it was demonstrated some time ago that the details of the form of well finished bowls, plausibly in use for the presentation and serving of food (though this has yet to be tested by lipid analysis), changed repeatedly through the earlier part of the stratified sequence at Vinča-Belo Brdo; rim forms, wall angles and carinations, for example, kept being adjusted (Schier 1996; 2000). Our ToTL modelling has added formal estimates for the rate at which this process unfolded; shifts can be tracked every one to two or two to three generations (Tasić *et al.* 2016a, 53).

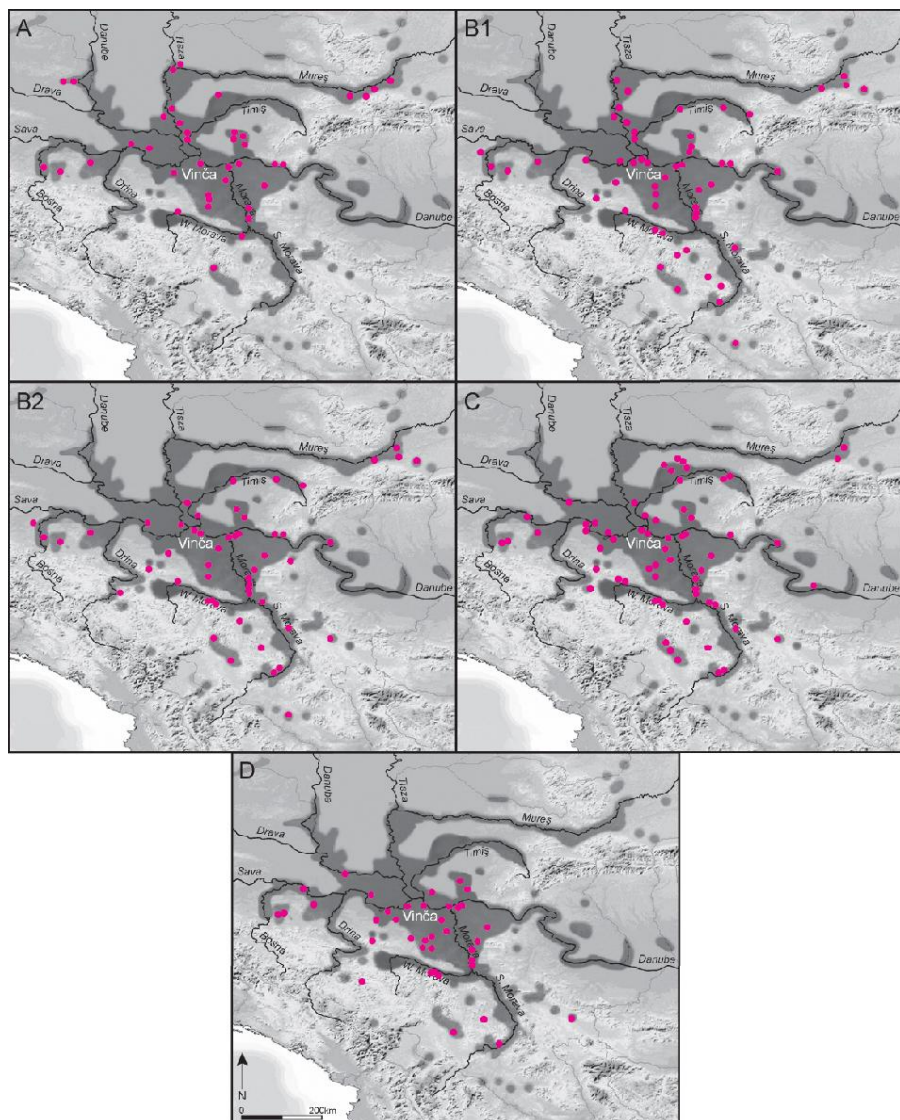


Fig. 4.27: The development of the Vinča network. After Whittle et al. (2016).

Our modelling concentrated first on the classic sequence from Vinča-Belo Brdo. Later on in our project we had the opportunity to date and model results from an intriguing situation on the northern limits of the Vinča culture distribution, in southwest Hungary. This was at the substantial settlement of Szederkény-Kukorica-duúlő, noted above. Here was an unusual combination of the groundplans of longhouses, of a kind more familiar in the LBK than in the Vinča world, and dominant Vinča pottery (Jakucs *et al.* 2016). This was of

the early Vinča A style, which dominated the assemblages from the eastern and central areas of the site. The western part contained mainly Ražište-type pottery, a style best known from areas immediately to the south in northern Croatia and probably an early manifestation of the still poorly understood Sopot culture. There was also some LBK-style pottery in all areas of the site.

The Szederkény settlement probably began in the later 54th century cal BC (Jakucs *et al.* 2016). That estimate turned out to be particularly interesting, with implications far beyond getting the details of culture history right (important though that is). We also gathered up existing dates for the formative and earliest phases of the LBK (among others, following Hungarian terminology), and formally modelled the chronology of the early development and spread of the LBK in central Europe as a whole. Previous informal estimates had placed the start and spread of the LBK in general as around 5500 cal BC, or in some accounts even earlier. Our models suggest that the first appearance of the LBK as represented by the formative style of western Hungary and north-east Austria probably did begin around then, but that the main diaspora of new things and practices in the earliest LBK into central and western Europe, and very probably people as well, probably did not occur until the middle decades of the 54th century cal BC (Jakucs *et al.* 2016, figs 17–18, 19–20, 21–22). In fact, the start of the LBK diaspora and the appearance of the first Vinča pottery appear to have been effectively synchronous (Whittle *et al.* 2016, 29, 41). So whatever the reasons for both the beginnings of the Vinča pottery style and the emergence of the Vinča complex, and the start of the great LBK diaspora, both took place in conditions of considerable change, and at first sight it seems plausible that people were actively creating new identities through the manipulation of material culture.

We also reviewed the corpus of existing other radiocarbon dates for the Vinča complex as a whole (Whittle *et al.* 2016). There were some 600 of these. Around 500 were directly usable, and as in any exercise of this kind, the samples varied in quality and in their association with diagnostic Vinča pottery, through the five principal phases established long ago by Milošević (1949a: much debated and refined since, as fully referenced in Whittle *et al.* 2016). Without going into all the details here, what we claim on the basis of the formally modelled chronology of the Vinča ceramic development across its broad distribution is a much better sense of the timing and tempo of the emergence of a network, whatever the other factors in its formation including the possibility of the movement of people (Fig. 4.27). Contrary to older notions which straightforwardly envisaged new people coming in

large numbers from the south, from the southern Balkans and even Anatolia, we posited a more complicated mixture of people.

The distribution of the Vinča A phase appears more weighted to the north than to the south (though that does not entirely exclude some movement of people from the south, but this will require further aDNA research (cf. Hervella *et al.* 2015; Mathieson *et al.* 2017; and see further [Chapter 6](#)), which will not be easy given the general lack of Vinča human remains). The network appears to both intensify and expand through the B1 and B2 phases, over a period of a couple of centuries or more, reaching its peak in the Vinča C phase. An alternative earlier explanation for the Vinča C style had been in terms of a cultural shock, produced by the arrival of further new people from the south (Garašanin 1951; Lazarovici 1981; Suciú 2009), but from our perspective this is better seen as the time when the network was at its most extensive and intensive.

Dušan Borić (2015a, 158) has proposed seeing the implied connectivity in terms of ‘various influences, borrowings, emulations, exchange, trade and breeding networks’, and I have emphasised above that such processes were probably played out principally in household settings. There is a strong impression of a gradually intensifying but longlasting and therefore stable system of social interaction. In such a system, it looks as if material culture was actively used by people through the river valleys of the northern and central Balkans to foster a sense of shared practice and presumably thereby identity, though there is no need to insist on a single or uniform ‘Vinča identity’.

However stable the Vinča system may have been for centuries, it came to end in the middle of the fifth millennium cal BC; our estimates for the ending of occupation at Vinča-Belo Brdo itself suggest the 4540s or 4530s cal BC (Tasić *et al.* 2015). Dušan Borić (2015a) has suggested a series of endings in the 47th and 46th centuries cal BC, which our formal models support (Whittle *et al.* 2016); the main occupation of the Uivar tell, as just one example, probably came to an end around or soon after 4700 cal BC (Draşovean *et al.* 2017; Whittle *et al.* 2016, 14; Schier *et al.* forthcoming). Various explanations for this unravelling have been suggested (reviewed in Borić 2015a), but focus on the possibility of conflict among independent households. Now this period in ceramic terms is Vinča D, and our estimates suggest considerable overlap with Vinča C (Whittle *et al.* 2016, fig. 37). This might suggest a widening of choices of ceramic style made at household and community level, but one of the many puzzles of this situation is that there is no obvious further material differentiation in the later stages of the Vinča complex, either

between houses within individual settlements or from region to region. That contrasts with the striking regionalisation in the latter stages of the LBK. In line with what I have argued in this section, perhaps this reflects continuing allegiance to long established connections and values within a by now old network, despite factors and processes at work which led to its ending. In that scenario, people could have gone on actively asserting particular kinds of relationships through their use of material culture, until the bitter end.

The ‘Danubian’ tradition in the upper Rhine valley

I have already sketched earlier in this chapter the many shifts in numbers and locations of settlements in the Lower Alsace region of the upper Rhineland, through the long Neolithic sequence from the LBK of the later sixth millennium cal BC to the end of the Danubian tradition in the late fifth millennium. Our formal modelling indicated that changes were constantly happening, probably occurring at intervals of two or three generations, though interrupted by the suggested major hiatus between the LBK and the Hinkelstein style (Denaire *et al.* 2017). That gives the opportunity to follow the track of changes in pottery, seriations of which lay at the heart of the high-resolution chronology proposed. This is largely a study of changes in decoration, since in broad terms the repertoire of bowls and jars, though altering in detail with time, remained much the same in broad character across the generations. Unlike in the archaeology of the Vinča complex, this is not a record where pots can be located in houses or even on intact house floors, since those do not survive for the LBK and the Danubian tradition of the region. Many vessels come from pits, though including the longpits flanking house plans, and there is no reason to doubt that they were in constant use in the flow of daily life. Pots were also included as grave goods, in recurrent positions around the body (Bickle and Whittle 2013, chapter 8).

In the conditions of initial settlement in LBK IIb to LBK III in Lower Alsace, our formally modelled date estimates indicate a relatively rapid series of shifts in the decorative motifs on pottery (Fig. 4.28), and this can plausibly be seen as part of the social conditions in which settlement was established by a new population in a new region (Denaire *et al.* 2017). It was no doubt important for new identities to be established and reinforced, with decorated pottery, putatively involved in processes of social interaction beyond the households where it was created, for example through the sharing of food, an important means of signalling both affiliation and difference. As settlement continued and reached its greatest numbers in LBK IV, there were further significant changes in pottery (Denaire *et al.* 2017).

The decorative repertoire expanded considerably. ‘Traditional’ angled decoration now competed with an orthogonal ‘T’ style and horizontal ladder motifs. The choice of style seems to be made at the scale of the household. It is possible to distinguish in varying quantities assemblages dominated by the ‘traditional’ decoration, assemblages dominated by the ‘orthogonal’ decoration, and ‘mixed’ assemblages, in which the two compositions appear in equal proportions (Jeunesse 2008; Lefranc 2013). Again pottery seems central to social interaction in conditions of relatively rapid change, including the introduction of large cemeteries.

I do not need to detail all the steps in the Danubian tradition after the resumption of settlement in the Hinkelstein phase, probably from the first half of the 48th century cal BC onwards (see Denaire *et al.* 2017). After initial reestablishment in the Hinkelstein phase, a full occupation of the landscape seems to have occurred in the Grossgartach phase, probably from the later 48th century cal BC. In this instance, whereas seriation had suggested five phases of ceramic development, our formal chronological modelling suggested that the stylistic differences largely overlapped. The now rather diverse motifs are found in the same areas and on the same sites, but not in the same pits or graves. This again seems compatible with an intense concern with identity at a number of scales, in another phase of change, with pottery being used for active signalling. Things seem more settled in succeeding (and longer-lasting) phases, until more regional differentiation appears in the later fifth millennium (Denaire *et al.* 2017).

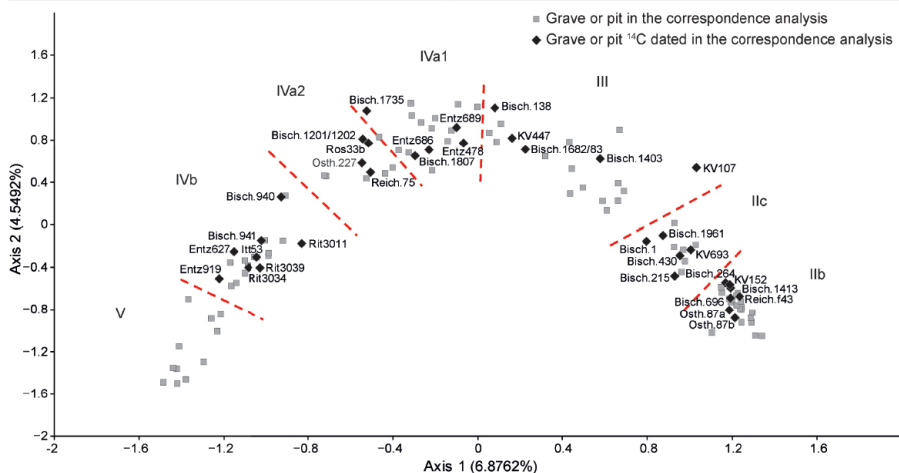
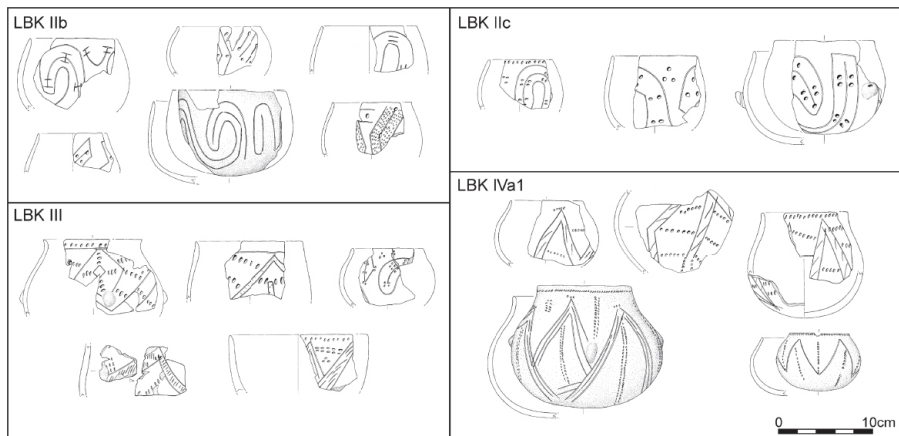


Fig. 4.28: LBK (Rubané) pottery from Lower Alsace. Top: selected forms; below: correspondence analysis of the sequence. After Denaire *et al.* (2017).

Grooved Ware in Orkney

A new kind of pottery, Grooved Ware, appeared in the settlement context of the Late Neolithic in Orkney, probably in the 32nd century cal BC. The time was one of considerable changes in the Orkney islands (as already discussed above). According to other modelling within the ToTL project, both larger stalled cairns and the architecturally elaborate Maeshowe-type passage graves may have appeared at roughly the same time, around 3400 cal BC (Bayliss *et al.* 2017). Stone-built houses may also have begun to be more common in the latter part of the fourth millennium (Richards and Jones 2016; Griffiths 2016), and associated with Grooved Ware more settlements, some of them of considerable size, become visible in the landscape, across a wider range of locations (Richards *et al.* 2016). A period of overlap in the later fourth millennium cal BC is indicated between an older style of round-based pottery and the novel, flat-based Grooved

Ware (Bayliss *et al.* 2017). It is often suggested (e.g. Sheridan 2016) that Grooved Ware, which in various forms is distributed across much of Britain and now parts of Ireland, could have originated in Orkney, partly because associated dates from Orkney appear to be earlier than from elsewhere and partly because the greatest quantities of this ceramic have so far been excavated in Orkney. Those are certainly strong arguments, but they need to be put in context. More research has been carried out on Orkney than on most other regions, and variants or ‘sub-styles’ of Grooved Ware are found widely in mainland Britain, with pots (especially in the so-called Woodlands style) which match ones in Orkney being found in both eastern and southern Britain. That could indicate a wider network of innovation rather than a single point of origin (Richards *et al.* 2016, 221). A further complication is that we still do not understand all the conditions of the emergence of Grooved Ware.

The sequence from Pool on Sanday, within the Orcadian archipelago, shows the gradual development of flat-based and then recognisably Grooved Ware pottery. It was not possible to date the lowest part of the sequence, but its phases 2.2 and 2.3, with a range of flat-based forms and incised decoration, belong between the 31st and 28th centuries cal BC (MacSween *et al.* 2015). That development could be taken to stand for a gradual emergence of the new style across the islands as a whole, but subsequent modelling of the chronology of Barnhouse (Richards *et al.* 2016; preliminary dating and modelling for Ness of Brodgar support this case: Card *et al.* 2017) shows a well-established Grooved Ware style already in existence by the later 32nd century cal BC. Modelled estimates for the introduction of the Orkney vole also fall in the later fourth millennium (Bayliss *et al.* 2017, fig. 4), and we have speculated whether there could also be a connection between the widespread occurrence of flat-based pottery in western Europe in the later fourth millennium (in both the Horgen culture of the Alpine foreland and the *Néolithique récent* of northern France) and developments in Orkney (Bayliss *et al.* 2017).

More precise chronologies therefore help to both define the shifting context and outline some of the competing options for the emergence of Grooved Ware in Orkney (though it will take much further research across Britain as a whole before these can fully be resolved). Whichever explanation is preferred, whether of an insular or a wider phenomenon, it seems clear that this development was deliberate, and created by people, rather than the outcome of some kind of cultural drift (Richards *et al.* 2016, 220; Sheridan 2016, 200; Sheridan *et al.* in prep.). Novel pottery was one more way to create identities, but

served both to differentiate and to connect. Once established in Orkney, Grooved Ware appears to have developed quite slowly; our study has followed broad trends within 200-year timeslices (Sheridan *et al.* in prep.) (Fig. 4.29). Following the peak of settlement intensity and the suggested interruption to the previous core of Mainland occupation (Bayliss *et al.* 2017), a heavier style of Grooved Ware decoration emerged, perhaps to emphasise connection in a time of decline (see also Clarke *et al.* 2017).

The Alpine foreland: slow and rapid change

My last example takes us back to the Alpine foreland. What can the high-resolution timescales provided by dendrochronology, which I sketched in Chapter 3, offer to this wider discussion of the timing and tempo of material culture change? At first sight, for all the skill seen in the construction of buildings and in other crafts, whose remains are visible thanks to organic preservation, much of the pottery of the Neolithic sequence of the Alpine foreland sequence (Stöckli 2009; 2016) is frankly underwhelming, with a muted range of forms and little decoration. A contextual approach, however, can make sense of this (Ebersbach 2013; Ebersbach *et al.* 2017; D. Hofmann 2013; Hofmann *et al.* 2016). If foreland pots are rather dull to our eyes, the setting in which they were used was dynamic. The vast majority of settlements appear to have been in use for short periods of time, and a strong case can be made for shifting and flexible attitudes to residence and membership of particular places of residence; that contrasts, however, with much longer-lasting tenure of locales and tasksapes, in which individual occupations regularly came and went (Ebersbach 2013; D. Hofmann 2013; Hofmann *et al.* 2016). Broad ceramic style zones seem to emphasise connection rather than difference (Ebersbach *et al.* 2017, 3–4).

This can be suggested as an accepted social strategy. Even when examples do occur of more marked differences within individual settlements, which dendrochronology helps to show were exactly contemporary with the ‘normal’ ceramic repertoire, they exist alongside regular practices. Whatever now the precise date of Arbon-Bleiche 3, most of its pottery belongs to a kind transitional between the Pfyn and Horgen styles. But there is also pottery of Boleráz style, an early phase of the Baden culture of the eastern end of the Alps and into the Carpathian basin. This can make up as much as 30 percent of all potsherds found in one single square metre without showing clear concentrations within single houses (de Capitani *et al.* 2002, 216, Abb. 318). Most of the foreign-style pottery appears to have been produced from local clay. There are also some sherds with Bavarian parallels,

mainly in Altheim and Cham styles (de Capitani *et al.* 2002, 217–20). These ceramic differences and preferences appear to have been assimilated within a broad spectrum of practices evident across the site as a whole (Doppler 2013; Ebersbach *et al.* 2017).



Fig. 4.29: Grooved Ware pottery in Orkney: a decorated vessel from the late midden infill of Structure 14 at Ness of Brodgar. Photo: Nick Card and Roy Towers.

Another example appears to show much the same flexibility, but over a longer timescale. Concise sous Colachoz on Lake Neuchâtel has a long sequence through much of the fourth millennium BC, well dated by dendrochronology (Burri 2007). The local pottery style was that of the Cortaillod. This dominated the ceramic assemblages, but there were again pots in non-local style, akin to those of the *Néolithique Moyen Bourguignon* (NMB) found over the Jura hills to the west in eastern France, and some seen as a hybrid between the two. A narrative has been suggested (Burri 2007, 169–75) of the successive arrivals of women from the NMB area who practised their own potting traditions, without, apart from some hybridisation at intervals, affecting the continuity or character of the local Cortaillod style. That plays out through the long history of the repeated occupation of the site, and again speaks to an established social strategy of flexibility and assimilation (Ebersbach *et al.* 2017).

A final Alpine foreland example of people making choices and

asserting identities through material culture comes in the horizon when Corded Ware was adopted, replacing the Horgen style. Dendrochronology suggests that this took place in northern Switzerland, quite rapidly, in the 28th century BC. In the Lake Zürich area, this could have happened in one or two generations around 2750 BC (summarised in Ebersbach *et al.* 2017). Now elsewhere in central Europe and beyond there are good grounds, provided by increasing aDNA analysis (Haak *et al.* 2015; Allentoft *et al.* 2015; Kristiansen *et al.* 2017; and see [Chapter 6](#)), for seeing new population coming in with Corded Ware, though the diversity of the complex as a whole can still be stressed (Furholt 2014; 2017). New population has been a popular explanation for Switzerland as well, but there are many continuities in the Alpine foreland in important aspects of daily life, ritual practices and settlement organisation, as well as in material culture other than pottery, for example axes. A total change of population here seems unlikely (Hafner and Suter 2003, 36; Ebersbach *et al.* 2017, and references), whereas a local population may have chosen to adopt new material, rapidly, in a wider context characterised elsewhere at least in part by change and population movement. In western Switzerland, in contrast, the development around 2750 BC is different. Instead of the whole production of pottery being changed, local traditions carry on side by side with Corded Ware pottery. Furthermore, elements of Corded Ware pottery are ‘translated’ into something mostly resembling local pots, but with some Corded Ware style decoration or with a flat instead of a curved base (Stöckli 2009, 114, Abb. 68). As in the north, settlement traditions remained unchanged in western Switzerland – and burial customs, whatever they were, still remained mostly invisible, in marked contrast to the situation in the wider Corded Ware distribution (Ebersbach *et al.* 2017).

Things in time

In the examples above, I have referred several times to people actively creating and using pottery as part of various social strategies. This is largely a practice-based approach (Pauketat and Alt 2005; Harris and Cipolla 2017, 38–9). If I think that there is a convincing case for seeing pottery as being very actively used by people in the projection of identities and the maintenance of relationships, from a theoretical and comparative point of view, this chimes best with the view of human life as formed by a system or systems of relationships, and as metamorphic, causal and interactive, as proposed by the anthropologist Michael Carrithers (1992, 27, 31). He has outlined ‘a setting in which change might be thought of as natural, the setting of

actual social life with all its fluidity, uncertainty, construals and misconstruals, its laboriously achieved continuity, its planned and inadvertent innovations' (Carrithers 1992, 36). What he has also called the 'culture project' involves 'the finding and displaying of variations in the cultural rhetorical resources which people use on themselves and one another to establish a scene, make a movement, and lead to a performance' (Carrithers 2010, 167). This puts people's actions, choices and values centre-stage.

Many would now disagree with this kind of position, which could be dismissed as an old-fashioned humanist stance (Harris and Cipolla 2017, 162). With the material or ontological turn, a people-centred view has been challenged, across several disciplines (Witmore 2014). As Yvonne Marshall and Benjamin Alberti have put it (2014, 19), 'an ontological turn is underway ... worldviews are being discarded in favour of worlds'. A constellation of ideas has been involved, or 'entangled genealogies' (Barad 2007, fig. 34). These include – and I make no claim to cover everything systematically or comprehensively (for a more detailed review, see Harris and Cipolla 2017) – Actor-Network Theory, with things as well as people operating as 'actants' (Latour 1993; 2005); vibrant matter and vital materialism (Bennett 2010); object-oriented ontology (Harman 2016); assemblages, again including the material world as well as people (Deleuze and Guattari 1987; DeLanda 2004; 2006; 2016); bundling (Zedeño 2008; Pauketat 2013); intra-activity, agential realism and ontological unity (Barad 2007); the dwelling perspective, which has both Heidegger and Merleau-Ponty behind it (Ingold 2000), and meshworks (Ingold 2007; 2011); the 'lively knotting' of all the constituents of the world (Haraway 2007); entanglement (Watts 2008; Ogden 2011; Hodder 2012); the secondary agency of things (Gell 1998; cf. Robb 2010; Witmore 2014); and notions of relational personhood (Strathern 1988; Fowler 2004). This kind of approach, in which any 'separation between an active social world and passive material residue' (Barrett 2014, 67) is denied, has been quite rapidly and increasingly widely adopted within archaeology, following a first call for a 'symmetrical archaeology' (Webmoor 2007; Witmore 2007). Books and papers have covered (and again I make no attempt to be exhaustive) not only things, encompassing materials as well as artefacts (Olsen 2010; Hodder 2012; Alberti *et al.* 2013; Witmore 2014), but also animals (papers in Watts 2013b; Betts *et al.* 2015), birds (Overton and Hamilakis 2013) and plants (van der Veen 2014; cf. Kohn 2013), and a relational approach has also been extended to ancestors (Velasco 2014), bodies (Marshall and Alberti 2014), rock surfaces (Jones 2014)

and water (Strang 2014).

It is important to emphasise how diverse this set of approaches is; some of the many differences are underlined by Harris and Cipolla (2017). Not all these notions necessarily go together; Tim Ingold, for example, has suggested SPIDER in opposition to ANT (2011, [chapter 7](#)). Graham Harman (2016, 14–16) challenges axioms of the ‘new materialism’ (see again Witmore 2014), with alternative axioms of ‘immaterialism’, arguing for example that not everything is contingent, that change is intermittent and that ‘what a thing is turns out to be more interesting than what it does’. Nor is a single linear account of development easy, though it has been claimed from an archaeological point of view that notions of entanglement may be taking precedence over the notion of material agency (Watts 2013a, 12). In another way, there is the risk of the same kind of ‘flat ontology’ (DeLanda 2004, 58) being applied over and over again, without little or no regard to specific situations, or the possibility of variation and change; these new worlds can often appear as timeless and static contexts. Comparatively few of the most influential and general theorists are archaeologists or directly interested in archaeology as such, and there is the familiar problem of how our discipline should absorb ideas developed initially for other purposes. Some of the recent statements by major figures are either very general, not to say bland (for example, DeLanda 2016, [chapter 1](#)), or ill-focused in terms of case studies on material and material sequences themselves (for example, Harman (2016), who chooses to regard the Dutch East India Company as an ‘object’ (Harman 2016, 33) and who focuses, albeit within a chronological framework, more on its ‘ontology’ than on its ‘history’ (Harman 2016, 40)).

That is not to say that all such studies need be similarly diffuse. In my reading, less generalising accounts are much more focused and much more alert to specific moments of change, as for example in a study of shell trumpets in the American Southwest (Mills and Ferguson 2008). There is no reason why Bayesian chronologies could not enhance studies of sequence in a relational framework, such as given in British studies by Chantal Conneller (2011) for the Mesolithic or Chris Fowler (2013) for the Early Bronze Age. The outcomes, however, will be very different. In directly challenging an anthropocentric view of the world, this bundle of relational concepts also undermines a conventional understanding of historical narrative (at whatever chosen scale or scales) of the kind I have been exploring in the chapters so far. Christopher Witmore, for example (2014, 21), explicitly refers to archaeology taking leave of its long-standing self-

definition as the study of the human past through material remains.

That is why it is important for my overall argument to note critique of the ontological turn. John Barrett has supported the value of keeping a distinction between different qualities of humanness (Barrett 2014, 68) and between living and non-living things (Barrett 2014, 70, 72); he has argued that the agency of things is too indeterminate (Barrett 2014, 69). In a further paper (Barrett 2016, 1684), he has reasserted the ontological distinction between 'living and non-living matter' and importantly argues that the 'return to things' has nothing to say about 'the dynamics of historical process'. In the introduction to a volume of papers which partially endorse the 'ontological turn', Ruth Van Dyke (2015) reviews in depth many of the key strands, but notes that there has so far been little space given to putting ideas into practice. Beyond that, she also stresses that 'we run the risk of turning away from people, and we abrogate responsibility for intentional action' (Van Dyke 2015, 7). She ends with a spirited defence of 'anthropocentrism' (Van Dyke 2015, 20), refusing to duck the 'thorny issue of intentionality' (Van Dyke 2015, 19); her vision is of 'objects as part of a lived aesthetic continuum that affects, enables, and constrains meaning and behavior' (Van Dyke 2015, 21). The conditions which fostered a turn to things have also been seen to stem from a 1980s 'crisis of representation', making it easier to talk about things rather than people, and salvaging and amplifying the authority of Western scholars (Fowles 2016).

There is also a sense in many of the archaeological and other sources quoted above of a universal kind of perspective being advocated, and that can seem closest to an animist or animic view of the world (Ingold 2007; 2011; Scott 2006; Bird-David 2006; Harvey 2006). According to this, all humans are agents, but there are other agents than humans alone (Ingold 2013, 246; Harvey 2006, xi); 'other-than-human' persons may take many guises (Hallowell 1960). But that raises the difficulty that distinctions can be made within animic or animistic worldviews, and that animism can also be distinguished from, for example, totemism, shamanism and 'perspectivism' (Bird-David 2006; Ingold 1998; Descola 2005). If, by contrast, we revert to the view that there 'really' is some kind of fundamental, underlying relationship between people and other constituents of the world, that ignores the fact that many people, emically, have seen the connections in very different ways, at different times and in different places. Even within the nexus of approaches being discussed, there are divergent views on what constitutes material agency. Though a 'flat ontology' is asserted by some (DeLanda 2004, 58; van der Veen 2014, 809,

claiming ‘equal agency’ among people, plants, animals, material culture and environment), it is not universally agreed that people, animals and things act or have effects in the world in the same ways (Ingold 2011, 89–94; Watts 2013a, 7). Oliver Harris and John Robb have argued (2012) that multiple ontologies can exist and emerge in any given context.

Nonetheless, there are abundant ethnographic examples of people ascribing life force and animacy to things, from sacred bundles on the Plains of North America (Zedeño 2008; Pauketat 2013, [chapter 3](#)) to houses (Waterson 1990, 115–21; 2013; Creese 2012, 365). Of potential relevance to the pottery case studies which I have given earlier in this chapter, in the American Southwest there are ethnographic and historical accounts of the Hopi and Zuni which suggest that pots are regarded as animate, beginning with a belief in the sacred character of the earth in general and the properties of clay in particular (Chiyskowski 2015, 89). Further, it is the process of making a pot, rather than the vessel itself, which animates the result (Chiyskowski 2015, 90–2), and this is argued to apply to everyday plainwares as well as to special or ritual painted vessels. But among the Zuni it is noted that not all clay is equal and that some sources are seen as more powerful than others (Chiyskowski 2015, 90), while different pots have varying effects, depending on ‘gender, age, ritual, or status’ (Chiyskowski 2015, 92).

Again there are the obvious potential dangers in the use of analogy and of over-generalising, even within one cultural setting, let alone when ideas of this kind might be applied universally across all manner of contexts. Oliver Harris has taken pains to resist the universal imposition of animist perspectives (Harris and Cipolla 2017, 205). But it is worth returning to the ceramic case studies above to see what such a perspective might add. In Vinča house interiors, for example, there is no obvious spatial separation of ‘special’ from ‘mundane’, and there could be a case for seeing all their contents as lively elements or members of an animated house. There are other clay objects as well as pottery, which could well have been regarded as animated. Figurines, prosopomorphic lids – classically in the form of catlike faces – masks and small legged vessels often classified as altars are obvious candidates. A notable recent discovery was of a set of 46 human figurines from the late Vinča house 1/2008 from Stubline, just noted above (Crnobrnja *et al.* 2009). Some of these appear to be carrying tools or weapons. These were placed in small groups on a small platform on one of the long walls near one end of the 9-m long, single-roomed house ([Fig. 4.30](#)). Opposite it was a clay setting classed as an

altar, and down the same side was a quern. There were two ovens, one immediately adjacent to the figurine group, one prominent storage jar and two concentrations of pottery, one inside and one outside the building; next to the pottery group inside the building was a modelled clay bucranium.



Fig. 4.30: The figurine group from House 1/2008 at Stubline. Top: detail of context as excavated; middle: groups as excavated; lower: the ensemble as a whole. Photos: Adam Crnobrnja (and see Crnobrnja et al. (2009); Crnobrnja (2011)).

Likewise, I have argued elsewhere that we could think of the LBK grave and the body it contains as echoes or mirrors of the longhouse, and thus symbolically powerful (Whittle 2012). Pots could have been included simply for contents of food and drink, but more plausibly as presenting aspects of the life of the deceased, including the ability to

provide sustenance and hospitality (just as stone axes could be read in a similar fashion). Similarly, in Orkney, Grooved Ware pots large and small are found both in the carefully composed domestic interiors of houses and the largest buildings such as Structure 8 at Barnhouse and Structure 10 at Ness of Brodgar, and in a range of chambered tombs; pots so large that they could not have been carried in or out of narrow doorways, for example at Skara Brae (Clarke and Shepherd in prep.), might have been thought of as residents or occupants in their own right, animate members of the lively setting of houses. There is frequent decoration on the walls of the Ness of Brodgar buildings (Card and Thomas 2012), and there is broad overlap between the repertoire of Grooved Ware pottery decoration and the range of motifs employed in the passage tombs of eastern Ireland, plausibly to enhance if not also animate the experience of using those great structures. But there also seems no compelling reason to exclude the agency and intentionality of people, in first creating a new style of pottery, and then using it to signal identities both locally within Orkney and at much longer range (though in ways that we do not easily understand) over much of Britain and Ireland. The same can be argued for both Vinča and LBK trajectories, and also for the varying contexts of the Alpine foreland. In my view, the possibility that individual elements of material culture could have been regarded as animate hardly subverts sequences of decisions and choices made by people.

Sometimes sudden and sometimes slow

This long survey of aspects of settlement, monuments and material culture has been deliberately detailed, to underline what formal chronological modelling can bring to their study – from whatever theoretical perspective – and to indicate what still remains to be done better in future research. Presumably the precision available in dendrochronologies will never be matched by the kinds of modelling discussed here, but the gap is closing. Different timescales begin to open up more frequently from these kinds of study, with narratives now routinely possible (though not necessarily uniformly so) at both lifetime and generational scales. That allows us to discriminate between the sudden and the slow with much greater confidence, and to unpick the constituents of long-term processes of change with much greater sensitivity. Practically everywhere across the studies reviewed in this chapter I see much more dynamic narratives of actions, decisions and choices made by people beginning to emerge than we had previously envisaged for Neolithic lives. I go on in the next chapter to consider how that can also contribute to our understanding

of community and leadership, and in the chapter after that to reflect on what all this can tell us about Neolithic history as a whole.

Notes

¹ Childe's still useful if somewhat vague term.

² Optically Stimulated Luminescence dates. For further detail, see Bayliss *et al.* (2017).

³ In more detail, the formal modelling (Osztás *et al.* 2016b; Bánffy *et al.* 2016) suggests that burial activity in subsite 5603 probably began in 4790–4740 *cal BC* (68% probability) and that it began at a similar time, 4795–4745 *cal BC* (68% probability), in subsite 11 (Fig. 4.11). An intensive period of burial began slightly later, in 4715–4690 *cal BC* (68% probability), on subsite 10B. At this time settlement was established across a wide area, in subsite 11 from 4745–4690 *cal BC* (68% probability), on subsite 5603 from 4745–4665 *cal BC* (68% probability), and on subsite 10B from 4720–4700 *cal BC* (68% probability). After a brief episode of intense occupation, lasting at most a few decades, settlement and then burial ended on the northernmost subsite 10B, in the 4700s or 4690s *cal BC* (68% probability) and 4695–4670 *cal BC* (68% probability) respectively. Settlement also ended before burial on subsite 11, but endured for much longer. The settlement here ended in 4670–4620 *cal BC* (37% probability) or 4610–4565 *cal BC* (31% probability) and burial in 4585–4515 *cal BC* (68% probability). Both settlement and burial endured longest on subsite 5603, although here the end of burial preceded the end of settlement by well over a century. Burial ended here in 4515–4465 *cal BC* (68% probability), and settlement ended in 4345–4245 *cal BC* (68% probability).

⁴ Borrowed from the title of a poem by Dylan Thomas.

⁵ In a lecture at the 1992 TAG conference. The idea is discussed throughout his survey of the Balkan Neolithic (D. Bailey 2000), though the phrase is not used.

Chapter 5

Time lords? Community and leadership across the Neolithic sequence

What kind of social place?

So far, I have considered the varying scales at which we can investigate the past, interrogated the kinds of chronology which we can now construct, and examined, through a range of case studies across themes of settlement, monumentality and materiality, the precision now available to us when formal chronological modelling is properly combined with other sources of information. I have argued that with radically enhanced chronological precision archaeology can begin to be more closely aligned with history, on the one hand through a shared concern with narrative, and on the other in a desire to deal with particular situations rather than being content to operate with generalising models. That potentially represents a major shift in how we conceive of the discipline of archaeology as a whole, not just Neolithic studies. In this chapter, I want to go on to ask big questions of the nature of Neolithic sociality, from a historical perspective in the sense I have defined. How can we characterise particular situations, and what sort of labels should we apply to them? Should we be content to simply slap on labels from the comparative literature, or does that betray a willingness to operate with generalisation? Those questions define the substance of this chapter. What kind of a history was this? Should we seek in it coherent pattern, continuity and steady change, or do we have to do with more fractured narratives, which could include reversals, collapses and hiatus? What can that tell us about the kind of social place, or places, that Neolithic Europe was?

Before we come to a series of case studies, I want to look at various aspects of past approaches to these kinds of question, not for textbook completeness or historiographical coverage for their own sakes – much of what I will cite is well known – but in order to argue, first, that the range of answers offered in the wider comparative literature has diversified substantially, which makes the choice of approach more rather than less difficult; and secondly, that the majority of these

possible answers remain general models. So the difficult challenge, thirdly, is to begin to build a much more nuanced understanding of particular times and contexts, and in that task appropriate attention to timescales is a key requirement. My main claims will be that no major differences were established or maintained for long in the course of the Neolithic and Copper Age in Europe, and that there is plenty of evidence for cycles of change, many of them rapid.

Past approaches and general models

It may seem unnecessary to go as far back as the work of Gordon Childe, Grahame Clark and Stuart Piggott,¹ but their rather cautious approaches, though showing a long-standing interest within archaeology in questions of the character of society and social change, served in part to condition the uptake of the much more optimistic, evolutionary models widely adopted in the early stages of processualism. As a first brief example, in *What happened in history*, Childe (1942) talked in very general terms of Neolithic ‘barbarism’ in both the Near East and Europe, and the ‘higher barbarism’ of the Copper Age, with food production as important for the ‘escape from the impasse of savagery’ (Childe 1942, 55). He also used the language of self-sufficiency and surplus, kinship, chiefs and kings; unusually large houses are ascribed to chiefs or chieftains, and barrows and ‘great stone tombs’ are mooted as ‘chieftains’ sepulchres’ (Childe 1942, 73), against a background of dominant kinship relations. Clark and Piggott’s *Prehistoric Societies* (1965) is notable for its near-total silence on any explicit aspect of society, while the treatment of sociality in the Neolithic in Piggott’s *Ancient Europe* (1965, 64) largely stuck to emphasising ‘Danubian peasants’, ‘peasant economies’ and ‘peasant communities’.

Against this backdrop, it was hardly surprising that the much more formalised and ambitious models promoted in the succeeding phase of processualist archaeology had such an impact. In the British literature, the prime example was the chieftdom model of Colin Renfrew (1973c) applied to the Late Neolithic of central-southern Britain, with its checklist of twenty criteria based on comparative, cross-cultural generalisations, and a background of social evolutionary typologies as advocated by Service (1962) and Fried (1967). A subsequent treatment, reflecting on his fieldwork in Orkney, refined the earlier stage of social development, with a simple model of segmentary society (Renfrew 1979). Stimulating though such models were on their first appearance, there was much to criticise; there was a tendency to treat social trajectories as akin to biological evolutionary processes, there was a great deal of generalisation, and it regularly proved

difficult, if not impossible, to assess many of the criteria proposed. But looking back, what is also striking is how approaches diversified rapidly from the 1970s onwards. In a first comment on the significance of the Varna cemetery, Renfrew rejected the excavator's proposal of some kind of state formation and tentatively assigned the situation to 'the chiefdom category' (Renfrew 1978, 202). But a later paper on Varna offered a much more sophisticated and open-ended discussion of subtle questions of value and wealth, and there is reluctance automatically to apply a chiefdom model without better knowledge of the regional context (Renfrew 1986, 130), even though the situation is assigned in general terms to complex, ranked societies (Renfrew 1986, 162). Likewise, a later paper on Late Neolithic Orkney included discussion of the possible role of pilgrimage, rather than focusing on rigid social models (Renfrew 2000).

That is to follow one author. More widely, early kinds of chiefdom models which stressed inevitable and unilinear processes, strongly driven by environmental and resource conditions, were quite quickly replaced by a host of other versions which emphasised much more active, agentic, managerial roles for people in diverse processes of change. The literature is wide, and well known, and I make no attempt here to review it systematically. Bob Chapman (2003, 33–70) has given a comprehensive and critical review; among many other sources, signs of what I see as broadening diversity can be found in Paynter (1989), Upham (1990), Price and Feinman (1995), Earle (1997) and Haas (2001). Although the majority of approaches to social characterisation in Mississippian archaeology, for example, have been lampooned by Tim Pauketat (2007) as stuck far too rigidly in notions of chiefdom and cycling between different versions of chiefdoms, classically between 'corporate' and 'network' modes of hierarchy (Feinman 2001), I think that is to underplay a welcome willingness to consider variation. We will come to European applications presently. Before that, I want to stress the diversification of models, not excluding those which propose very early examples of extreme social differentiation, and others which seek to dilute the significance of human agency, as part of the turn to things already discussed in [Chapter 4](#). I will note especially notions of cycling and resistance, both of which I believe are potentially very significant in considering the history of Neolithic European social development. On that note, however, I am in strong agreement with Pauketat's insistence (2007) on the need to construct histories, rather than rely on generalising models, however beneficially diverse.

As well as escaping monolithic notions of hierarchy (Feinman

2001), ongoing research has provided many examples of variation within what were at first seen as broadly uniform categories. Chiefdoms are now regarded as very heterogeneous, varying widely in scale, stability, organisation and control (and see R. Chapman 2003, 42–4, and references). Specific counter-examples to the initial cross-cultural generalisations of a strongly evolutionary approach can be cited, for example from Africa (McIntosh 1999, 4). Other categories have been proposed, of which the notion of ‘transegalitarian’ societies is significant (Hayden 1995; 2001). Although proposed from what is described as an evolutionary perspective (Hayden 1995, 24), this is an important addition, as it inserts variation between previously rather simple categories of undifferentiated and differentiated society (‘neither egalitarian nor politically stratified’ (Hayden 1995, 18) and ‘societies with private ownership of resources and produce, low levels of sharing, and institutionalized hierarchies based ultimately on wealth (but also including ritual, kinship, and political dominance)’ (Hayden 2001, 232)). Further, ambitiously, it seeks diversity within so-called transegalitarian societies. With much emphasis on the generation of surplus, a general evolution is envisaged (Hayden 1995, fig. 2) from ‘despot communities’ to ‘reciprocators’ to ‘entrepreneurs’ (roughly equivalent to Great Men, Head Men and Big Men in earlier terminology: Hayden 1995, 25). This is variously driven by ambitious individuals or ‘aggrandisers’, using a variety of social tactics, including the generation and control of surplus, extortion, warfare, feasting (Hayden 1995, 31) and access to the supernatural (Hayden 2001, 261), some of whom are strongly ‘acquisitive, aggressive and accumulative’ (Hayden 2001, 255). Differences between these three categories are not absolute, but are based on varying combinations of features and tactics employed, such as a greater role for bridewealth among entrepreneurs or contrasting forms of feasting (Hayden 1995, fig. 4).

This generalising, evolutionary account is principally based on comparative ethnographic data, among which case studies from New Guinea and south-east Asia play a major part, but specific European Neolithic examples are briefly offered, but not explored in detail. Thus the LBK and ‘some Middle Neolithic settlements in Europe with fortifications, violent deaths, low population densities, and limited prestige goods all occurring in achieved status conditions’ are suggested for Despot communities (Hayden 1995, 41), while ‘European Neolithic causewayed enclosure communities where feasting, ritual, and sometimes violent conflict were prominent’ exemplify Reciprocator communities (Hayden 1995, 51); a wider

range of candidates is put forward for Entrepreneur communities, including ‘a number of early Neolithic “Old European” cultures in the Balkan area, early Megalithic communities in western Europe’ and ‘European Battle Axe and Beaker cultures’ (Hayden 1995, 62). We shall see reason to question many of these distinctions, and there is a real danger of setting up endless, frustrating identity parades, where the right suspects can never convincingly be picked out, even if they have all been rounded up.

Another example of the increasing diversity of social forms requiring consideration has come in the shape of the house societies model. Derived from Lévi-Strauss, this notion, in which the house becomes the embodiment of identity and the means of transmission and descent (Carsten and Hugh-Jones 1995; Beck 2007), has become very popular in archaeology. Claims for house societies range from the Neolithic of the Balkans and the LBK (Borić 2008) to the Late Neolithic in Orkney (Richards and Jones 2016). It is clear, however, that the model – whatever its anthropological validity and archaeological usefulness – could be applied to a wide variety of other situations, including in much later periods, so it is probably not to be regarded as some kind of recurrent phase or stage through which societies might pass.

The most wide-ranging and comprehensive recent discussion of social differentiation and the establishment and consolidation of power has been given by Kent Flannery and Joyce Marcus (2012) in *The creation of inequality*. Drawing on worldwide ethnography, this presents a very wide arc of variation, from clanless foragers to states, monarchies and empires. It does not explicitly set out an evolutionary perspective, but that is strongly implied by the order of presentation of the richly documented ethnographic case studies, by the chronology of the accompanying, selective archaeological case studies (in the latter, prehistoric Europe is almost exclusively avoided, which has been one of the spurs for my writing this and the succeeding chapter) and by the language of the final chapter. That includes questions such as ‘How did the old hunter-gatherer logic come to be changed, creating routes to renown?’ (Flannery and Marcus 2012, 552), and phrases such as ‘those cases where rank society did develop out of achievement-based society’ (Flannery and Marcus 2012, 554), which make it clear that while a plethora of individual situations are set out on a case by case basis, the implied overall schema is of some kind of developmental, evolutionary trajectory. That is open to the charge of a lack of specific, historical perspective (cf. Pauketat 2007; and see further below), but the sheer variety of circumstances which is offered

in the book is salutary reading for anyone attempting to characterise Neolithic European sociality. That diversity is impossible to summarise fully here; as a guide and a prompt to further reading, I set out some of the main elements in Figure 5.1.

Major steps or stages in the emergence and consolidation of social difference in the account of Flannery and Marcus include what they call inequality among foragers (for example through the creation of hereditary rank, and relative seniority among clans), the emergence of achieved renown among what they call achievement-based societies, the rise of hereditary inequality among chiefly or rank societies, and ultimately the consolidation of a stratification of social difference and the appearance of kingdoms and empires. It is important not to be lulled into thinking that there are single, neat or uniform lines of development. But for thinking about the European Neolithic, the variability of comparative social situations is highly significant, since that makes the task of applying neat labels or stages to the sequence more and more difficult. Secondly, Flannery and Marcus (like others before them) pose the question, over and over again, of why people would give up equality and allow differentiation, leadership and dominance to emerge; resistance (Flannery and Marcus 2012, 153) is a fundamentally important theme. Both these aspects deserve more detailed consideration.

The spectrum of kinds and strategies of differentiation is vast (see again [Fig. 5.1](#)). Even among foragers, Flannery and Marcus (2012, [chapter 5](#))² note senior lineages among the Chumash, with ‘chiefs’ or ‘captains’ who owned canoes, controlled access to hunting and foraging territory, and led war parties and ceremonies alike; hereditary chiefs among senior and junior lineages of the Nootka, backed by ancestry; and a wealth of moieties, clans and ranked lineages among the Tlingit. Among agricultural societies in New Guinea, a ‘hierarchy of respect’ is cited among the Etoro and Chimbu, with routes to social prominence open through feasting, raiding and exchange (Flannery and Marcus 2012, 95–9); below Big Men and ordinary men among Mt Hagen people was the category of ‘rubbish men’, often unmarried (Flannery and Marcus 2012, 101). Only one in five men among the Mountain Ok people were fully initiated, giving them considerable but not hereditary prestige (Flannery and Marcus 2012, 116; Barth 1987). Among the Angami Naga of Assam the ‘ladder of renown’ was climbed through prowess in headhunting, feasting and the achievement of holy man status (Flannery and Marcus 2012, 105–6). Further examples come from the Tewa, Hopi, Mandan and Hidatsa people of north America (Flannery and Marcus 2012,

chapter 9), among which can be noted a hierarchy of categorisations among the Tewa, endless bickering among the Hopi clans, and the scope for Mandan and Hidatsa individuals and families to acquire sacred bundles and the power attached to them, but balanced by social sanctions against the accumulation of wealth.

Major social categories	Kinds of social function	Characteristics	Selected examples
Egalitarian societies	Clanless foragers	Universal kin classification; sharing; generosity; non-accumulation; equal reciprocity; disapproval	<i>Arctic Netsilik; Basarwa; Hadza</i>
	Foragers with clans	Classification by descent; some group violence; cemeteries and ancestors; rights to foraging territories; moieties; seniority	<i>Andaman Is; c Australia</i>
	Inequality without agriculture	Leaders (chiefs) monopoly of exchange; kin ranking, individual ranking; feasts of merit; ancestral seniority	<i>Chumash; Kwakiutl; Nuu-chah-nulth</i>
Achievement-based societies	Unequal farmers	Autonomous villages; more emphasis on ancestral seniority; hierarchies of virtue; moieties, clans and lineages; headmen; some Big Men. Production, warfare and exchange all routes to differentiation. Renown but no real authority; no passing on of positions/titles. Men's/clan houses	<i>//Gana; Etoro</i>
	Balances between prestige/equality	Prominent people and positions but no hereditary elite. Again autonomous villages; clans; descent groups; raiding; some rich burials. But power sharing; sanctions of community; fissioning	<i>Naga; Mountain Ok; Solomons</i> <i>Tewa; Hopi; Mandan; Hidatsa</i>
Rank-based societies with hereditary inequality	Beginnings of hereditary inequality	Passing on of ritual authority; revision of genealogical record to legitimise seniority; combination of political and ritual roles; territorial expansion	<i>Avatip of Sepik River</i>
	Cycling	Shifting back and forth between hereditary privilege and equality; <i>gumsa/gumlao, ihendu/thenkoh</i> , changes in social logic inc by interaction with outside and debt slavery	<i>Kachin; Konyak Naga</i>
	Chiefly power	Combinations of religious authority, force, territorial expansion and exchange; life/force, expertise, toughness. Possession by birth. Status rivalry and cycling; rival/equal chiefs	<i>S Pacific; Panama; Colombia; Natchez Tikopia</i>
	Paramount chiefs	Multi-level political hierarchy; flows of tribute; checks in form of hereditary offices for advisors and war leaders	<i>Bemba</i>
	Aristocracies	<i>Mite/mura</i> (hereditary aristocrats/former slaves); <i>mite</i> clan ranking; control of agriculture; hereditary rank and wealth but no chiefs at all	<i>Apa Tani</i>
Stratified societies	Rulers and ruled	'Class endogamy'; barriers between strata; lengthy genealogies; linkage with the divine and ancestors; sibling marriage; elimination of landed gentry; warfare and expansion	<i>Tonga; Hawai'i</i>
	Kingdoms		
	Empires		

Fig. 5.1: The implied social evolutionary scheme of Flannery and Marcus (2012).

How was hereditary inequality achieved, given that equality is usually not easily surrendered and privilege normally challenged (Flannery and Marcus 2012, 208)? Flannery and Marcus (2012, 187–91) offer as an example of transition the case of the Manambu people in the village of Avatip on the Sepik river, New Guinea. Here was a complicated set of relationships among clans, subclans and lineages

(Harrison 1990). Power was dispersed. Political leaders came from the clan elders, who could exert secular power, especially through debate, while ritual authority was exercised by cult leaders. Subclans seem to have been the most active constituency, and secular and ritual leaders were rivals; ritual leaders who acquired political skills were envied, and at risk of being murdered (Flannery and Marcus 2012, 188). During the period of observation, one subclan was in the process of asserting a monopolisation of knowledge over the names of ancestors and their attendant secret myths; this had been going on for three or four decades (Harrison 1990, 176, 189; Flannery and Marcus 2012, 189). One potential route to power here appears to have been through control of genealogy.

The situation on the Sepik river was ultimately determined by colonial authorities, and no permanent elite emerged (Flannery and Marcus 2012, 191). In many other instances of course, processes of differentiation continued and ranked societies with hereditary leadership emerged. Not all these need be classed as chiefdoms (it is a moot point whether in the famous case of Tikopia the hereditary leaders are best regarded as chiefs or aristocrats: Flannery and Marcus 2012, 213), and as already argued the evidence suggests an enormous variety among those societies which can be called chiefdoms. For example, recurrent features of chiefly power can be identified among south Pacific societies, especially concepts of life force or supernatural energy (*mana*), expertise (*tohunga*) and toughness and bravery (*toa*), but these are often found in varying combinations: sacred authority with genealogical superiority, or political expertise with military force, or sometimes all together (Flannery and Marcus 2012, 209; Goldman 1970). Other examples of diversity in ranked and chiefly society include a series of historical situations in Panama and Colombia, which featured hereditary chiefs, an abundance of ranks, warfare, concepts of immortal essence and craft skill, and sacrifices of wives, concubines and subordinates at chiefly funerals (Flannery and Marcus 2012, 216–22; Helms 1979). The Bemba of Zambia as anthropologically observed in the 1930s (Flannery and Marcus 2012, 223–8; A. Richards 1940) had a paramount chief at the head of a political hierarchy, with district chiefs and subchiefs spread through a large population and wide territory. Chiefs were men, but from clans which reckoned descent in the mother's line and which were ranked by relative seniority. The paramount chief did not have untrammelled power, however, since that was shared with a council of elders in hereditary positions, and with hereditary war leaders (Flannery and Marcus 2012, 227).

Examples of this kind could easily be multiplied but their implications are already clear. In contrast to the early days when simplified schemes of social relations were introduced within an evolutionary framework, a far greater diversity of situations is apparent. At the very least, a much more complicated and nuanced narrative for social development in Neolithic Europe might be predicted. Given that very variability, context by context, it has also still to be argued that such potential analogies are relevant to any one historical case in the Neolithic of Europe. But a further highly significant implication of comparative studies is the recurrent evidence for constraints on power, levelling mechanisms and resistance, the effectiveness of which can be seen in the well documented examples of cycling, or the alternation between more pronounced and less developed forms of differentiation. Again, the relevance of this set of factors for the study of Neolithic Europe will have to be argued on a case by case basis, but awareness of it may be important in framing rather different historical narratives.

Perhaps one could cite recognition of resistance as far back as Childe (1942, 73), who mooted the existence of chiefs in a world dominated by kinship relations, but checks, balances and resistance appear to be more commonly cited as the literature on social development became more sophisticated, following early evolutionary approaches. Paynter (1989, 381) and Upham (1990; cf. Chapman 2003, 50, 57), for example, both specifically cited active resistance of moves to achieve power as a significant feature to consider in social trajectories. In one of the many volumes of that time, Trigger (1990; cf. Trigger 1976) gave a detailed account of the historical (and far from static or unchanging) Iroquois, with particular emphasis on how they were able to maintain egalitarian structures, especially through the active fostering of what Upham (1990, 10) called a 'communal ethos' or consensus (Creese 2016). The Iroquois had chiefs, many of whom were elected on the basis of achievement, but also hereditary peace and war chiefs from clan segments. As Trigger (1990, 133) puts it, 'the material benefits of chiefly office were more than counterbalanced by its expenses'. Leaders had to 'win public approval by exhibiting self-restraint, wisdom and generosity' (Trigger 1990, 135). Reputation or prestige could be achieved, by hard work or especially in warfare (Trigger 1990, 133), while the appearance of coercion was carefully avoided (Trigger 1990, 132). Chiefs had to be spokesmen for their groups (Trigger 1990, 137), and a rather atomistic social structure is described, of matrilineal clan segments embedded in a wider system of clans, phratries and moieties (Trigger

1990, 128). The checks and balances in all these and other arrangements and attitudes regularly resulted in factionalism, 'which asserted the continuing political independence of constituent groups ... in ever more precarious alliances' (Trigger 1990, 139).

Another study, of late Prehispanic western pueblo society, urged that both consensual and hierarchical social relations could be found in the same situation (McGuire and Saitta 1996). In their specific historical context, 'Prehispanic pueblos, while not egalitarian, were not stratified either, in fact, they were simultaneously *both*' (McGuire and Saitta 1996, 201; original emphasis). There was an ethos of egalitarianism, cooperation and peaceful relations. Village chiefs were only first among equals, while sodality chiefs or leaders provided a further balance (McGuire and Saitta 1996, 203), and households had cross-cutting and at times conflicting allegiances based on kinship, sodality membership and community obligations (McGuire and Saitta 1996, 204). I note the conclusion that 'the tension between equality and hierarchy conditioned an internal dynamic that governed the aggregation and fissioning' of late pueblo communities (McGuire and Saitta 1996, 209).

Commenting on the deficiencies of taking only aggrandisers into account, Polly Wiessner (2002, 233; cf. Wiessner and Tumu 1998) has stressed how varied are 'egalitarian structures and the coalitions that maintain them', which present significant obstacles to the institutionalisation of inequality. This was argued in a series of reflections on the study of historical process among the Enga people of New Guinea over a 250–400 year timespan, in which significant inequalities emerged with the development of elaborate exchange networks promoted by enterprising men, including the famous Tee cycle (Wiessner 2002, 234). Some of the resistance to transgressions against previously shared norms could be found in mundane practices of daily life: in everyday talk, serving as a levelling mechanism. That could also be bolstered by ostracism and accusations of witchcraft (Wiessner 2002, 235), the latter being found in many other situations (e.g. Hill and Gurven 2004). Social disapproval can be a powerful factor, limiting would-be aggrandisers to short-lived roles as 'little big men' or 'local despots' (Wiessner 2002, 236). The Enga case is of particular interest because of the estimated timescales. Historical traditions are reckoned to go back up to 12 generations, a generation here being reckoned as 30 years (Wiessner 2002, 237). At the start of things, there was a strong ethic of equality among both men and women (though not between them); some competition was permitted, and there were leaders of achieved status among lineages, subclans,

clans and tribes. Following the introduction of the sweet potato, settlement relocation to lower altitudes, which involved complex local migrations (Wiessner 2002, fig. 2) and ensuing population increase, changes began to appear, at first gradually, after three to four generations. This saw the first, small-scale practice over a generation or two of what developed into the Tee ceremonial exchange cycle, with the exchange of limited numbers of pigs, which were not to that date an important part of the agricultural economy, among a restricted number of partners (Wiessner 2002, 240). But over another generation or two, things became more competitive. Differentiating cults, land shortage, more hostile inter-group relations (the beginning of the so-called Great Ceremonial Wars) and an increased need for reparations all developed. The Great Wars were not all mock affairs, but seem regularly to have involved planned combats on chosen ground, in front of many spectators, with feasting and drinking at night (Wiessner 2002, 242–3). By the fourth generation before the present, the Tee cycle was expanded to help to finance this activity, and already in just one more generation, the ceremonial exchange cycle began to subsume and supplant ceremonial conflict.

In the course of this process (of which, for reasons of space, I have given a very simplified summary), different kinds of leadership emerged, over quite short periods of time. Tee ‘managers’ and Great War leaders were both superior to local clan leaders (Wiessner 2002, 247), but were not identical; positions in Great War leadership could be inherited, but not in Tee cycle management (Wiessner 2002, 246). The pace of change was measurably swift. Enterprising men early in the Tee cycle could effect the exchange of ten pigs, but four generations later some were able to assemble and distribute up to 250 pigs (Wiessner 2002, 247). The character of authority also altered. The historical traditions contrast an emphasis in early forms of leadership on personal traits and actions with more abstract and depersonalised presentation of authority in later generations, through ‘blackened faces, stance, grandeur, formal apparel, and eloquent oratory’ (Wiessner 2002, 247). Nonetheless, although these managers and leaders achieved wide reputation and control of institutions far beyond their own clans, the ethic of equality was maintained in other ways, in the continuation of access for all to the means of production and to the spirit world, of support from fellow clan members, and of freedom of labour and movement (Wiessner 2002, 247, 249). Not out of nothing in this context was the ‘road to success’ one of ‘distribution, not retention and accumulation’ (Wiessner 2002, 249).

The last stages of the documented historical process among the

Enga became entangled with European contact and then control, and it is impossible to know how things might further have developed – or reverted and collapsed. In other cases, we know of ‘cycling’ or alternation between different kinds of social relations. The classic case often cited is that of the Kachin of highland Burma (as was), as observed and interpreted by Edmund Leach (Flannery and Marcus 2012, 191–202; Leach 1954). Here, some Kachin societies, which were probably far from uniform, alternated periodically between an egalitarian mode, *gumlao*, and ranked inequality, *gumsa*. In *gumlao* times, lineages were kept in balance among other things by marriage rules, and villages were autonomous, while in *gumsa* periods, genealogical reckoning, underpinned by parts of their cosmology, came to the fore, and hereditary chiefs could control up to 60 villages, command tribute and have the power to call in debts (Flannery and Marcus 2012, 193–7). At least some of the dynamic of change may have been provided by relations with Shan neighbours, who had ‘princely states with lineages of aristocrats, commoners, and slaves’ (Flannery and Marcus 2012, 197). What is less clear are the timespans over which *gumlao-gumsa* cycling took place, though scales of generations seem to be implied (Leach 1954, 210).

Another example comes from not far away, among the Konyak Naga of Assam (Flannery and Marcus 2012, 201–6; Fürer-Haimendorf 1969). As observed in the 1930s, they too seem to have cycled between egalitarian social relations with achievement-based leadership, in *thenkoh* mode, and rank society with hereditary chiefs, in *thendu* mode, with raiding and headhunting as a frequently employed tactic for territorial expansion. It appears that arrangements were unstable and that change could happen both rather rapidly, and more slowly, as when chiefly families simply died out or were overtaken by population growth among commoners (Flannery and Marcus 2012, 203); timescales of generations again seem to be implied for the quicker changes (Fürer-Haimendorf 1969, 62–3). Other instances of such instability have been called ‘sequential hierarchies’ (Johnson 1982; Paynter 1989, 382), with ephemeral control and membership in flux, because ‘there are too many levelling devices, too many places to escape to, too many authorities, and too many weapons too widely spread throughout the population to foster accumulation of power by the few’ (Paynter 1989, 381). Wider discussion of ‘heterarchy’ (Crumley 1995) should also be noted.

Some previous European case studies

If in the light of the above we face increasingly difficult choices when it comes to interpreting specific European Neolithic situations and

sequences, how does the range of opinion and approach seen in the existing literature compare? My treatment is again selective – with examples in this instance mainly drawn from western Europe – and brief, but I hope it will be sufficient to underline the case for considerable rethinking of the character, tempo and trajectory of social change through the European Neolithic sequence. Many accounts of social change are framed within the kind of chronologies outlined in [Chapter 3](#), and many can be characterised as gradualist, their underpinning temporal frames almost automatically fostering a prevailing sense of slow, steady change. Many perhaps also in the end rely on a sense of social evolutionary schemes, though they vary considerably in the terminology employed and in the explicitness with which underlying assumptions are set out. I review a few of these,³ to draw attention to both the chronological precision or imprecision involved, and the kinds of models and interpretations on offer, which in turn condition the nature of the history being written for the development of European Neolithic societies. I will also note some interesting proposals which deviate from this broadly mainstream approach.

Two good examples come in accounts by Johannes Müller, who has set out a detailed scheme for the trajectory of social change in the Mittelbe-Saale region of eastern Germany (Müller 2001), and a parallel, more summary account of northern Germany and southern Scandinavia (Müller 2014; see also Müller 2010; 2011b; and Müller *et al.* 2012).⁴ The temporal frame for the former study comes from a combination of typo-chronological studies of the material and informal inspection of radiocarbon dates. Successive blocks of time are employed, usually rounded to the nearest hundred years, though occasionally to the nearest fifty years, and within these a social development is mooted which essentially uses the terminology of Fried (1967). On this basis, in the TRB I period in the region, from 4100–3800 cal BC, no social hierarchical differences are inferred, but in TRB II, from 3800–3500 cal BC, the appearance of substantial funeral mounds, flat graves with arrowheads and copper, the procurement of copper from the south, and large ditched enclosures are variously taken to indicate the possible emergence of a ‘new elite’, with a marked increase in ‘internal differentiation’ (Müller 2001, 445). In a diagrammatic ‘sociochronology’, this is classed as rank society (‘Ranggesellschaft’: Müller 2001, Abb. 261) ([Fig. 5.2](#)). This continues into TRB III and IV, from 3500 cal BC to the early third millennium cal BC, but from 3500 cal BC onwards some kind of crisis is mooted, related to difficulties in accessing copper supplies and seen

as expressed in regionally diverse ceramic decoration, and in due course 'new prestige links' in the Globular Amphora social system; at the same time there was further settlement expansion, from 3350 cal BC resulting perhaps in less importance for 'central' sites (Müller 2001, 445). Significant division of labour is inferred, and grave mounds with richly equipped individuals are seen to reveal 'inherited status positions of the dead' (Müller 2001, 446). This broad trajectory, which is seen to include 'historical diverse social dimensions from region to region' (Müller 2001, 446), is interrupted around 2700 cal BC by a wide set of changes associated with Corded Ware or Single Grave groups.

A parallel summary account of northern Germany and southern Scandinavia is also reliant on a combination of typo-chronological studies of the material (with some seriation: see, for example, Hage 2016) and informal inspection of radiocarbon dates, though formally modelled chronologies are available for a handful of sites (for example, Mischka 2011; Hage 2016). Four principal phases in blocks of centuries again serve to define major changes (Müller 2014, 207–8). Early stages are again seen as lacking 'marked social differentiation' (Müller 2014, 208), but from around 3800 cal BC non-megalithic long barrows mark 'socially outstanding individuals', while from after 3600 cal BC there is further 'societal change', with clearance and land taking, population increase, more conflicts, and the burial of 'obviously very special individuals' in non-megalithic structures and stone dolmens alike (Müller 2014, 208). Those contrast with the cooperative 'enterprise', 'ideology' and 'life style' seen in the construction and use of enclosures and in the destruction of material goods through deposition in remote places (Müller 2014, 208). By 3200 cal BC, passage grave burials strongly project a generalised notion of 'collectivity', though potential weapons multiply, ceramic regionalisation increases, animals may become 'associated with wealth' and male single burials may indicate 'a new group of distinct individuals within the social network' (Müller 2014, 208). This trajectory is again truncated, after 2800 cal BC, by the Single Grave culture.

Other, not dissimilar, gradualist and generalising accounts are common. In a summary narrative about the trajectory of change in northern France, the first emergence of power and markings of territory are found in Cerny monuments in the middle of the fifth millennium cal BC (Demoule 2007a, 57–9), while the first 'complex' societies are seen to follow between 4500–3500 cal BC (Demoule *et al.* 2007; and see below). Deliberately seeking longer-term trends, John

Robb (2007, 337) has summarised three main phases in the development of the Italian Neolithic and Copper Age. From the earlier sixth to the later fifth millennium cal BC social relations were based on the ‘heterarchical production of difference’, with identity ‘conceptualised in terms of locality and co-residence’; from the later fifth to the earlier fourth millennium cal BC identity was framed by ‘genealogical relatedness, with ‘generalised forms of value’ or ‘heterarchy based on potentially equal production of similarity’; and from the mid-fourth to the mid-second millennium cal BC, there was a similar value system, but with ‘more personalised expression with potentially prominent individuals thought of as apical ancestors of genealogical relations’, and with ‘genderised prestige’. Stability and the slow pace of change are emphasised (Robb 2007, 338).

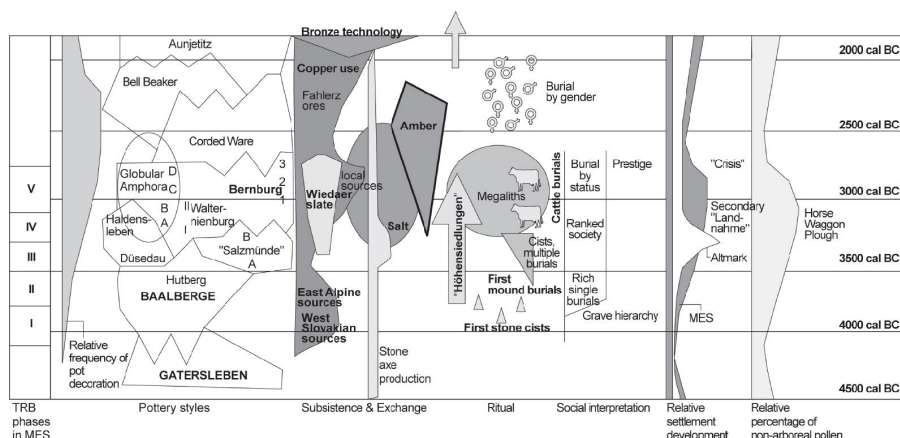


Fig. 5.2: ‘Sociochronology’ of the Neolithic sequence in the Mittelbe-Saale region (MES). After Müller (2001); translations: author.

Generalising chronologies are also routine in other, thematic treatments. A deliberately broad-brush account of the development of attitudes to the human body and its involvement in the creation and changing of social relations has split up the European Neolithic sequence into substantial blocks of time, variously 6500–4000 cal BC, 4500–2400 cal BC, 3300–2000 cal BC, and so on (Harris *et al.* 2013, table 4; see also table 3). Another discussion of the conditions in which wealth is argued to have emerged, in the form of ‘transportation infrastructure’, durable goods made of metal, and ‘capital investment in oxen used for traction’ is centred on a broad period in the fourth and third millennia cal BC, contrasting with ‘nearly two millennia’ of the practices and strategies of early farmers (Bogucki 2011, 109; see also the comparative generalisations on

intergenerational heritability among small-scale agricultural societies offered by Borgerhoff Mulder *et al.* 2009). Several of these concepts used risk being anachronistic, and ignore the earlier possibilities for the production, circulation and accumulation of valuables, including live animals in large herds; I will include issues of the so-called secondary products revolution in the next chapter. There is welcome reference to the tempo of change, with the assertion that ‘wealth quickly began to be accumulated’ once the three strands identified had begun to converge (Bogucki 2011, 113; cf. Heyd and Walker 2015), though just where and when is not spelled out in detail.

The above are just a selection from a much wider literature. My intention has not been to single out these various treatments for exceptional critique, but rather to underline, first, how routine it is to discuss social trajectories within chronological frameworks built mostly of blocks of centuries, with only occasional reference to more precisely defined horizons and to the tempo of change, and secondly, how the great majority of accounts adopt a more or less gradualist but progressive view of change. These two features are closely interlinked; a gradualist-progressive perspective is perhaps not directly entailed in a broad chronological framework, but it is certainly encouraged by it. Nor do I necessarily want to argue against all these characterisations of broad trends. Social relations and value systems may well have been stable or slow-changing in many areas, but without finer chronological resolution, there is the considerable danger of missing out, through lumping together, the ups and downs, the cycles, peaks and troughs, and the fine detail of the flow of life at lifetime and generational scales.

Probably the vast majority of discussions in the literature are couched in similar, chronologically broad or generalising frameworks, but not all adopt what I am calling a neatly gradualist and progressive view of social change. Interestingly, in the account of northern France sketched above, unexpected early differentiation is suggested for Brittany, as well as later simplification (Demoule 2007b, 84). Indeed, the results of ‘Projet Jade’ have highlighted the potential for unusual developments early in the sequence of Neolithic Brittany. The argument is based partly on ethnographic analogy with situations in New Guinea where there is long-distance exchange between individuals in stratified societies and where axes are the material expression of both the power of elites and ancestral power (Pétrequin 2012; Pétrequin and Pétrequin 2012), and partly on the combination of what are seen as unusually large concentrations of jadeitite objects of western Alpine origin in both non-monumental and monumental

contexts in Brittany, the latter including the very large and apparently early big mounds of St Michel, Tumiac and Er Hroek in the Carnac area (Cassen *et al.* 2012; Pétrequin *et al.* 2012). Without going into all the detail here, the argument leads to the striking proposition – so far little commented on in the wider literature (but see importantly Jeunesse 2014; 2017a) – that such a nexus of materiality and monumentality projected some form of divine or sacred kingship, promoted by the accumulation of unusual quantities of jadeitite objects, above all axes, and by the primordial character of the large Carnac mounds, set by the sea but evoking distant mountains (Cassen *et al.* 2012, 980–2). Whether such an interpretation is convincing is for detailed critique elsewhere, though I would want to worry about the choice of one kind of social label over another, about the magic number of axes that somehow transforms a potential ‘big man’ into a sacred king, and yet again about the chronology of the big mounds in question (see also Cassen *et al.* 2009).

In another early context, that of the LBK, attention has also been drawn to inherited status, in the form of well furnished child burials (Jeunesse 1997, 116), and to the possibility of an hierarchical ordering of LBK settlements (Jeunesse 1997, 120–1; see also Jeunesse 2017a). This leads to wider reflection on progressive or linear models of social change, challenging whether small Corded Ware graves, for example, necessarily reflect more inegalitarian relations than major constructions such as the fifth-millennium Passy monuments (Jeunesse 1997, 127), and questioning in general whether a more detailed and cyclical view of social change would not be more appropriate (Jeunesse 1997, 127). It is to be noted that other radical suggestions for the nature of LBK society, such as the possession of slaves (Gronenborn 2001), have not so far been adopted by the majority of researchers. In a later account, which emphasises the early development of political differentiation within a cyclical pattern of rise and fall, the possibility of slavery does not figure, though the existence of ‘Big Men’ is argued for the LBK, including from its earliest phases; although not explicitly furnished with slaves, such leading figures, both male and female, are seen as involved in warring and as having the power to punish others (Gronenborn 2016; Gronenborn *et al.* 2017).

A third example takes us in a slightly different direction. By the later fourth millennium cal BC in southern Iberia, what is known as the Copper Age emerged. This can be defined not only by the first production of copper and gold but also by a series of other developments, including the architecturally distinctive tholos tombs

and their sometimes exotic contents, and a range of large sites, variously defined by ditches and walls, or by concentrations of features, buildings and tombs. My specific example (see [Chapters 1 and 4](#)) of a large site that stands out in its regional context has been the complex at Valencina de la Concepción in the lower Guadalquivir valley in south-west Spain (García Sanjuán *et al.* 2013a; 2017; 2018); others include the famous site of Los Millares in south-east Spain, with its bastioned walls, ‘fortlets’ and external concentration of tombs, Camino de Las Yeseras, Marroquíes Bajos, and Zambujal and Perdigões in Portugal (Chapman 2008; Kunst 2010; Cámara Serrano *et al.* 2012; Valera *et al.* 2014; Valera 2017; Balsera Nieto *et al.* 2015; and references). Even that incomplete list is enough to suggest considerable diversity. Valencina has both more modest tholos tombs and a number of impressively large examples, including Montelirio, La Pastora, Matarrubilla and possibly Ontiveros (Fernández Flores *et al.* 2016), some lengths of deep ditches, a host of subsoil features, a range of ‘artificial caves’ and other mortuary contexts, evidence for copper production and a range of domestic activity, as well as a spectrum of objects in materials of far distant origin, including cinnabar, amber, ivory, gold and rock crystal (García Sanjuán *et al.* 2013a; 2017; 2018). How then to characterise such phenomena, even if they are demonstrably varied?

One response, perhaps more or less in line with standard or gradualist expectations of Neolithic and Copper Age social trajectories, has been to see these varied situations as evidence of more marked differentiation than in earlier times, with competition among emergent elites and between lineage groups, but in an unstable context (R. Chapman 2008; García Sanjuán and Murillo-Barroso 2013; Díaz-del-Río 2011). Thus it is argued that there is no convincing evidence for institutions that overcame ‘self-regulation mechanisms that worked against inequality’ (García Sanjuán and Murillo-Barroso 2013, 134) and that we can see ‘the rise of some kind of corporate groups (maybe lineages), made possible and sustained through the cyclical involvement of different segments of society in collective labor processes’, with lineage competition channelled into the mortuary sphere (Díaz-del-Río 2011, 51; see also Gilman 2013); another account refers to village-based Millaran chiefdoms (Ramos Millán 2013). Despite some recognition of cyclicity, as quoted above, the great majority of discussions have been conducted within the very broad chronology for the southern Iberian Copper Age summarised by Bob Chapman (2008).

Much of this opinion, in turn, is opposed by varied claims for some

kind of early state-like formation in Copper Age southern Iberia (Nocete Calvo 2001; López Aldana and Pajuelo Pando 2014). Social evolutionary theory allows the concept of ‘the state’ to be rather varied, derived from diverse processes, and not necessarily rare (Yoffee 2005, 196, 231). In southern Iberia, the same evidence, for example from Valencina de la Concepción, is read in terms of extreme differences: ditches seen as fortifications, objects acquired from distance as tributary flows and centralised investment (Nocete Calvo 2001, 86), and funerary variations as clear signs of asymmetrical society, ‘within which one cannot dismiss the possible existence of slavery’ (Nocete Calvo 2001, 99). One particular strand of evidence is treated very differently, since the early state model is accompanied at Valencina at least by claims for very high levels of copper production and accompanying environmental degradation (Nocete Calvo *et al.* 2008).⁵ How Copper Age sociality is regarded can be seen as at least partly bolstered by views of what came after. There are plenty of proponents of even more developed social differentiation in the Argaric Early Bronze Age, at least in south-east Spain (reviewed in R. Chapman 2008; Gilman 2013), even though even here a verdict of ‘political entities along the lines of the hillfort chiefdoms of the Maori ... or the pre-Incaic Peruvian highlands’ (Gilman 2013, 22) is more convincing to me than that of a single state or series of small state-like formations. And in similar fashion, with reference to Bronze Age models far beyond Iberia, we can note arguments over whether the chiefdom is a useful concept for late third and earlier second millennium cal BC societies in western and central Europe (Earle 1997; Kristiansen and Larsson 2005; Brück and Fontijn 2013).

Finally, all the contributions to debate about Neolithic sociality reviewed so far appear to agree on one thing: that this is a discussion about human agency and interaction. With the long reach of the ‘ontological turn’, that too has now been challenged. Thus, to take just one example, for the third-millennium setting of Late Neolithic Avebury and Silbury Hill, it has been argued that we have failed to take into account ‘the full range of actors/actants (both human and nonhuman)’ (J. Pollard 2013, 179), and should rather distribute agency across ‘heterogeneous relationships that take in complex collectives of humans and nonhumans (be it things, places, animals, spirits, and so forth), knowledge, practices’ (J. Pollard 2013, 183); what we call monuments are to be seen as ‘meshworks that variously drew together flows of substances, cosmic power, people, histories ... into moments of creation and form’ (J. Pollard 2013, 185). But while we are to understand them ‘as in a constant state of coming into

being’ (J. Pollard 2013, 185), no specific chronology is proposed for Avebury, though Silbury Hill is recognised as belonging to ‘a very particular set of conditions present during the third quarter of the third millennium’ (J. Pollard 2013, 191), and the general assertions of the argument are backed by the analogies of Polynesian marae and associated concepts of *mana* and *tapu*. The concluding emphasis from this perspective is on ‘flows of matter, ideas, people, and spiritual agencies’ rather than on the ‘the unfolding of events and constructional activities’ (J. Pollard 2013, 191–2). A generalised rather than specific narrative of politics and power relations appears to win out.⁶

Seven case studies

How can fine-resolution chronologies contribute to our understanding of all these issues? My case studies are brief and selective, drawing mainly but not exclusively from the *Gathering Time* and *The Times of Their Lives* projects. Most of these have already been mentioned or outlined in earlier chapters, but I use them again here to try to draw out insights into the nature of social relations and the character of change. In the light of discussion in this chapter so far, I am particularly interested in the tempo and duration of social differentiation, and in the challenge of how to characterise it, without resorting to predetermined labels.

The Vinča world: the connectivity of a long history

As already set out in [Chapters 1](#) and [4](#), the Vinča phenomenon lasted for several centuries, from the later sixth to the mid-fifth millennium cal BC. In some ways, one might be tempted to regard the outcome of the formal modelling of the chronology of both the complex as a whole (Whittle *et al.* 2016) and of individual sites like Vinča-Belo Brdo (Tasić *et al.* 2015; 2016a; 2016b) and Uivar (Draşovean *et al.* 2017; see also [Chapters 4](#) and [6](#)) as simply a useful refinement of what we essentially already knew about duration and continuity. I believe, however, that the formal modelling has thrown up unexpected insights and important implications not just for the Vinča region but for both south-east and central Europe as a whole. As noted already, very few other comparable chronological studies currently exist (one thinks here of Okolište, Csőszhalom and Pietrele).

Modelling has highlighted the conditions of both the start and the end of the Vinča sequence and it has helped to open up what constitutes duration and continuity. I have already offered a set of generalisations about the major characteristics of the Vinča phenomenon as a whole, and it would be easy to see these various

changes within a gradualist perspective, as the next, perhaps inevitable step in the development of a Neolithic way of life, following the early centuries of the Starčevo and related cultures. Modelled date estimates for the start of the Vinča A phase in general, and for the start of the accumulation of Vinča deposits at Vinča-Belo Brdo, however, both indicate something more dynamic at the beginning of the sequence, probably from the later 54th century cal BC. There may well be local discontinuity at Belo Brdo itself between the Starčevo and Vinča deposits (Tasić *et al.* 2016a; 2016b), Vinča beginnings more widely coincide with other striking changes represented in the wider spread or diaspora of the LBK (Jakucs *et al.* 2016), and Vinča beginnings could be seen not necessarily as straightforwardly the outcome of population movements from the south, as in older explanations, but as part of a set of shifts in settlement density throughout much of south-east Europe in the later sixth millennium cal BC, with some local or regional population displacements not excluded (Whittle *et al.* 2016).

The extended biography of the Vinča-Belo Brdo tell (Fig. 5.3) and the steady spread and intensification of the Vinča network then speak strongly to stable social relations. There is no obvious evidence for overt hierarchical differences within the great majority of tell or flat sites, not helped by the general lack of mortuary evidence. Tells and large flat sites may themselves speak to uneven social relations between communities. John Chapman (2000, 208–11, 215–16) in particular has used the abundance of ritual paraphernalia and exchanged items in the early and middle parts of the sequence at the Vinča-Belo Brdo tell as evidence for its regional dominance, and correspondingly, an alleged reduction of such material as a sign of its decline in the later part of its life. In fact, formal modelling has shown a more or less steady rate of tell accumulation throughout the biography of the site, and the argument about quantities of material could be disputed. Not only did the Belo Brdo tell continue serenely to rise ever higher, generation by generation, but it became a very large site (Tasić *et al.* 2015; 2016a; 2016b). It may therefore have been a dominant place for centuries.

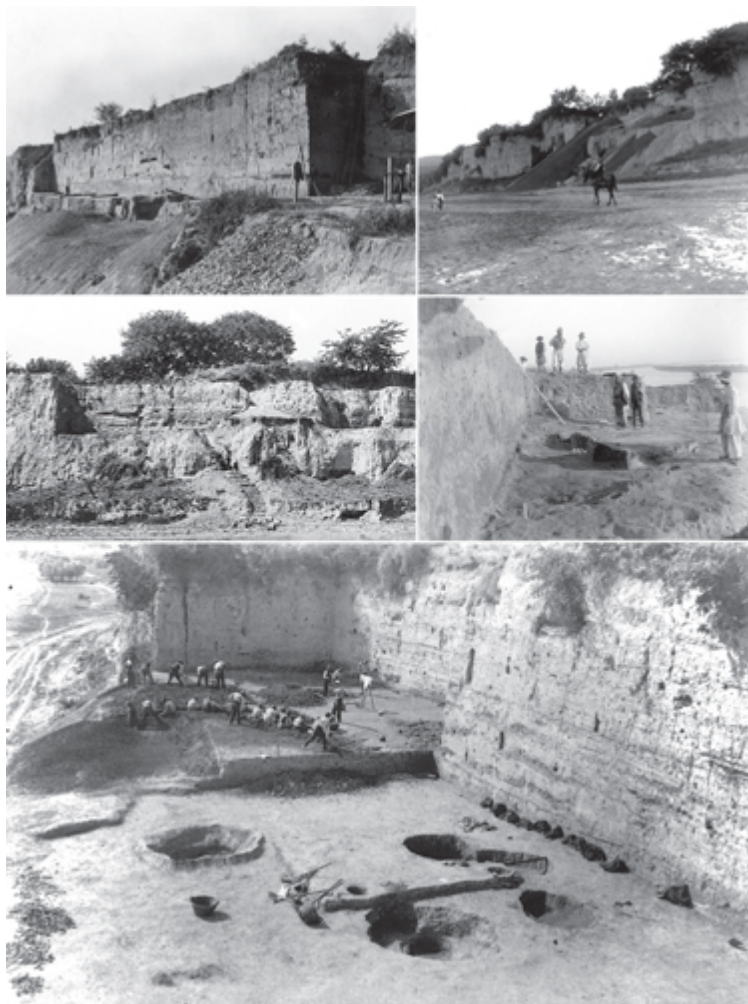


Fig. 5.3: Evoking the scale of the Vinča-Belo Brdo tell, from scenes of the Vasić excavations. Photos: Faculty of Philosophy, Belgrade University.

Stability could also be reflected in possible patterns of house replacement and burning. The argument could be made that the middle centuries of the Vinča culture sequence witnessed fewer house burnings than the early and late stages of the complex (Tasić *et al.* 2016b), and this putative phase coincides with the greatest intensification of the cultural network which the expanding Vinča culture represents (Whittle *et al.* 2016); correspondingly, there is some evidence that burning episodes were more frequent in early and late stages. On that basis, a sustained period of peaceful conditions could be seen as having accompanied and perhaps as having largely made possible the maintenance and growth of the Vinča phenomenon. These claims require both disclaimers and further explanation. The number of formally modelled site chronologies remains very small; I am

drawing here principally on the formal estimates for Vinča-Belo Brdo and Uivar (Draşovean *et al.* 2017), with an eye too on Okolište in Bosnia in the related Butmir culture (Müller *et al.* 2013; R. Hofmann 2013) (Fig. 5.4).

Interpretation of house burning is of course full of unresolved difficulties (among others, see Tringham 1991; 2005; J. Chapman 1999; Stevanović 2002). Scale and intention are central but very challenging questions (Tasić *et al.* 2015, 1077–9). Can we distinguish between individual or wider house burnings? Can we discriminate between accidental and deliberate burnings, and if the latter, between differing motivations such as aggressive acts among and between households and communities or the ritual and symbolic ending of individual households, say at the end of household lives or on the death of household leaders? Ruth Tringham has used ‘domicide’ for aggressive acts, and coined ‘domithanasia’ for ‘voluntary’ endings (2005, 107). She concludes in favour of ritual or symbolic burnings of individual buildings. She does not exclude accidental burnings that got out of control, nor burnings caused by local jealousy or wider, inter-communal aggression, but circumstantial evidence and theoretical, comparative considerations are summoned to set them aside. The very high temperatures measured or inferred from daub deposits, along with the nature of experimental burnings, are used to downplay the likelihood of accidental burnings, and a claimed lack of burning in the space between houses is employed as an argument against extensive burnings, while from general considerations negotiation is asserted as likely to have been more effective and more widely employed than short-term violence (Tringham 1991, 122; 2005, 103–6). However, at all three examples of Belo Brdo, Uivar and Okolište it seems clear that burnings could involve many more than individual buildings. We have therefore argued that some burnings at least were probably, because of their scale, some associated evidence for human deaths, and the provision of ditches interpretable as defensive at Uivar and perhaps Okolište,⁷ the result of aggressive action by others, presumably from outside the immediate community (Tasić *et al.* 2015, 1078).

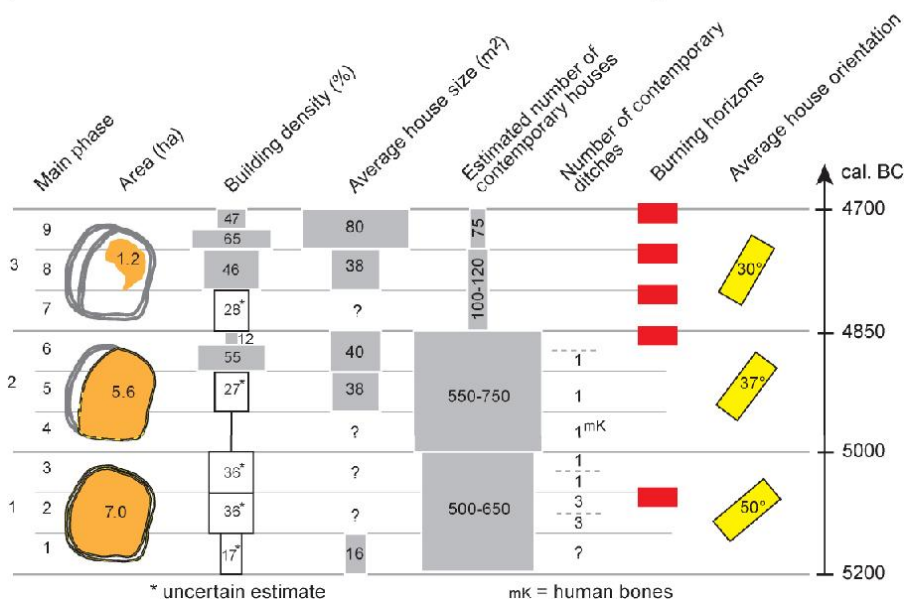
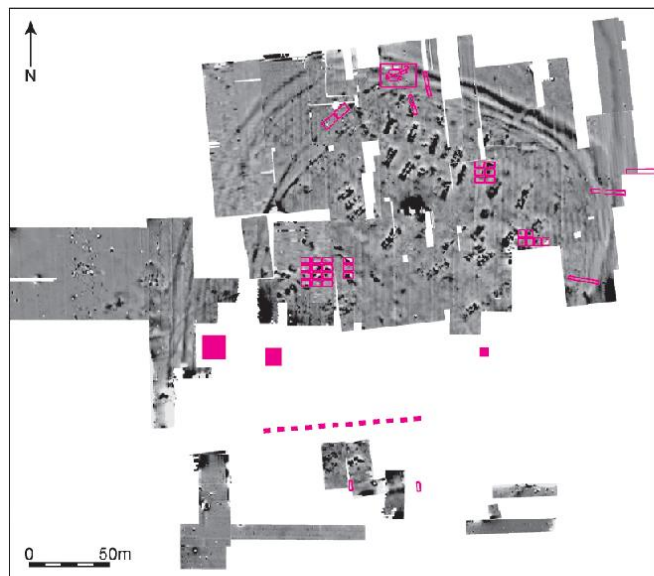


Fig. 5.4: The Okolište tell. Top: geophysical survey and main excavation trenches; below: selected features of the sequence. After Müller et al. (2013), by kind permission of Johannes Müller; translations: author.

This may help to define the general conditions of Vinča existence, but is far from specifying the nature of sociality in this context. In discussions of earlier fifth millennium Hungary and then later of Vinča-Belo Brdo itself, John Chapman (1997; 2000, 214) has referred to tells as ancestral timemarks and arenas of social power, operated by limited interest groups. That points at differentiation of some kind, but does not pin it down. At one level, there seems little doubt that

Vinča sociality was played out in face-to-face settings, probably in the context of individual houses. It appears that the shapes of pottery bowls, which were arguably used in the presentation and sharing of food in domestic contexts, were continually changing every couple of generations or so (Schier 1996; 2000; Tasić *et al.* 2016a). Modelled estimates for the duration of individual houses show fairly considerable variation, but also suggest an average duration of one to two generations (Bayliss and Whittle in press, fig. 4), and I believe that we can conclude that the individual house – and perhaps the individual household – was very much a finite and temporally limited social entity. The house presumably therefore took its place in a wider context, grouped and often literally aligned with other very similar buildings in contemporary neighbourhood settings, and both underlain and overlain with predecessors and successors in a much longer temporal succession.

So the formal modelling achieved so far helps not only to refine the timings and span of the long Vinča history, but also to begin to prise open some of its constituents. There is probably no easy analogy or readily applicable label from the array surveyed earlier in this chapter. The communal or corporate dimension of Vinča existence is compelling, though not all communities were equal, and the longevity of major places is striking, though clearly not all big sites lasted for the same amount of time. But whereas many early villages elsewhere were ‘social tinderboxes’ (Wilshusen and Potter 2010, 178), in the case of early Mesa Verde examples in the American Southwest rarely lasting more than one to three generations, this was emphatically not so in the Vinča situation. Could the analogy of some African societies help further to draw out the character of the Vinča complex? Susan McIntosh (1999, 4) has discussed African ‘societies in which central authority, often of a ritual nature, is paired with a power structure that is often diffuse, segmentary, and heterarchical, as well as societies in which considerable complexity is achieved through horizontal differentiation and consensus-based decision-making’. Further, ‘the distribution of power among several corporate entities (e.g. lineages, secret societies, cults, age grades) can be regarded as a strategy that has successfully resisted in a variety of ways the consolidation of power by individuals’ (McIntosh 1999, 4). Now it is part of the overall argument of this chapter that we should move away from interpretation led by general models or analogies, but the kind of perspective set out by McIntosh can encourage us to read the specific Vinča evidence in fresh ways. In particular, it could embolden us to bring back to centre stage the abundant evidence for ritual in the form

of figurines, altars and masks, which appears to have been a feature principally of domestic contexts (see again [Fig. 4.28](#)). Household ritual centred firmly in the domestic sphere may have been a major mechanism by which any tendencies to centralising differentiation in the Vinča world were actively resisted.

In the latter part of the Vinča sequence, modelled and better founded results help to refine existing views of processes of change. Dušan Borić (2015a), using a mix of formal and informal date estimates, had already identified the 47th and 46th centuries cal BC as the main period of abandonment of Vinča settlements. The models now available for Vinča-Belo Brdo serve to pin down the last use of the tell probably to the 4540s or 4530s cal BC, and two successive burning episodes, only some 25 years apart, seem to be part of the circumstances of this ending (Tasić *et al.* 2015; 2016b). We are not aware of any abandonment that is later than this. Likewise, the endings of both Uivar and Okolište around 4700 cal BC (Draşovean *et al.* 2017; R. Hofmann 2013) serve to define the time when abandonments started in earnest. It is also the horizon when not far away in south-west Hungary, but in a different setting and tradition, the major Lengyel aggregation at Alsónyék was at its brief peak of most intense activity (Osztás *et al.* 2016b; Bánffy *et al.* 2016). In all three cases – Belo Brdo, Uivar and Okolište – endings can be read as individual events, but that still leaves a period of a century and a half over which closures and change took place: a slow decline at the end of a long history.⁸ I have already noted in [Chapter 4](#) the apparent lack of regionalisation in Vinča D pottery (Whittle *et al.* 2016)

The nature of the Vinča ending should also speak to the character of what came before. Various models of environmental or economic pressure have been proposed (reviewed in Link 2006), and there are others which rely on a culture-historical narrative of population change (reviewed in Borić 2015a). Others favour a general sense of grit in the social oyster, of potential tensions and decline (J. Chapman 2000, 216–17); there is evidence at many sites, not just Belo Brdo and Uivar, for final burnt horizons (Borić 2015a, 157). One important model has been that of the rise of the autonomous household, in the context of population growth, with potential conflict between senior and junior lineages and the consequent budding off of smaller settlements (Tringham and Krstić 1990; Tringham 1992; Tringham *et al.* 1992). There is some evidence for individually larger houses in late Vinča contexts (Tripković 2007; 2010), and some late outlying, off-tell buildings at Uivar were substantial (Draşovean *et al.* 2017), but overwhelmingly the individual house appears to remain embedded in

neighbourhood and community settings, as noted in [Chapter 4](#) for Stubline among other examples. Undoubtedly we need many more precise chronologies for individual late Vinča sites. Then not only could the biographies of specific places be picked over, but also regional trends be examined in more detail. Did a combination of both inter- and intra-community competition begin to develop in an increasingly full or even crowded landscape, especially in the outer parts of the Vinča social network, while life in the core of the system persisted still for another six to eight generations or so? Once previously stable social relations began to unravel, not only in one context but in a series of adjacent settings, such as seen in south-west Hungary in the story of Alsónyék, or in eastern Hungary (Parkinson 2002; Borić 2015a; Raczky *et al.* 2015), a series of changes in both the nature of the connectivity of the network and the efficacy of local communal or corporate values could have been set in train. But such had been the robustness of the Vinča system that it took a century and a half or more to shift to the very different circumstances of what is known as the Early Copper Age (Parkinson 2002; Borić 2015a).

Varna: the brief construction of difference?

The Varna I cemetery takes its place in a longer sequence of settlement of the eastern, coastal part of Bulgaria, probably beginning in the sixth millennium cal BC (D. Bailey 2000), with cemeteries probably appearing here and in south-east Romania by 5000 cal BC, if not earlier (Borić 2015b; Stratton *et al.* in press); the much larger cemetery of Durankulak spans the run of the Hamangia and Varna cultures (Todorova 2002; Windler *et al.* 2013).

The salient facts of the context of the Varna cemetery and its contents are well known (summarised, with references, in Whittle 1996, 96–101; see also Higham *et al.* 2007; 2018; Leusch *et al.* 2014; 2015). There are still only scanty traces of local settlement, though a series of well known tells exist in the wider region. The cemetery contains over 300 graves, of men, women and children, substantial numbers of them with quite modest grave goods; it is worth remembering the substantial number (over 40) of cenotaphs or graves without human remains. What has provoked so much interest of course have been the contents of a lesser number of graves and empty graves, containing spectacular objects, especially in gold, but including also copper, shell, bone, stone and clay, used for an array of ornaments, containers, tools and weapons. The man in grave 43, and the empty grave 36, both in the southern part of the burial ground, are amongst the most abundantly furnished ([Fig. 5.5](#)). Further study and research have served to underline the exceptional nature of the

site, since nothing quite like it has emerged in the region or beyond since its discovery in the early 1970s; it is also clear that impressive skill was applied in the manufacture of stone beads (J. Chapman 2014, 401), as well as in the production and crafting of the gold (Leusch *et al.* 2014; 2015), and the presence of jadeitite axes in graves 43, 51 and 153 links the site with the great exchange network of western Europe, even though two of those objects cannot be precisely sourced (Pétrequin *et al.* 2012a; 2012c, 1277; J. Chapman 2013, 324).

There is also the matter of date. Previously assigned on typological grounds (which should not be overlooked) to a later part of the regional Late Copper Age (Todorova 1995), two rounds of radiocarbon dating and formal modelling, including the application of modest marine offsets, suggest an earlier dating, from the 46th to the 45th or 44th centuries cal BC, with a probable overall duration of just under 200 years (Higham *et al.* 2007; 2018; Chapman *et al.* 2006). So how does this chronology affect our interpretation of the significance of the Varna graves? We can note that the latest estimate for the start of the Varna cemetery in the 46th century cal BC coincides with the date estimate for the abandonment of the Vinča-Belo Brdo tell (Tasić *et al.* 2015, fig. 10). Though obviously in a different setting, and not least because established tell and related settlement continues, after the Varna cemetery, into the later fifth millennium cal BC (D. Bailey 2000; a specific, dated example is Pietrele in the lower Danube: Reingruber 2015a), does this coincidence say something more general about the possibilities of and opportunities for social differentiation at this time? I argued over 20 years ago that it was easy to over-read the Varna evidence (Whittle 1996, 96–101). From an early stage, as noted earlier in this chapter, there was talk of chiefdoms and even some kind of state-like polity (Renfrew 1978; Ivanov 1991). I had tried, perhaps unsuccessfully, to suggest a more subtle position, to resist automatically ascribing modern values to the presence of gold (see also Renfrew 1986), and to think of how the empty graves could have promoted ancestral veneration, for example, rather than straightforwardly have marked social ranking.



Fig. 5.5: Gold in the Varna graves. Top: a new reconstruction of grave 43, following excavation records: © Varna Regional Museum of History; below: the contents of empty grave 36: © Barbara Armbruster.

One popular view remains that the abundance and nature of the grave goods reflect very pronounced social differentiation (e.g. Chapman *et al.* 2006; Higham *et al.* 2007; 2018; J. Chapman 2013). This kind of view concentrates on the difference between the graves and cenotaphs within the cemetery. In purely descriptive terms, four ranks are proposed in terms of abundance of grave goods (the highest with seven or more categories of grave good, the lowest with one or none) (Higham *et al.* 2018). In other accounts, a threefold classification of ‘lavish’, ‘rich’ and ‘poor’ is used, only some 10–15 graves exemplifying the first (Chapman *et al.* 2006, 172; Higham *et al.* 2007, 649). These are then translated into social types, but the ascriptions vary, demonstrating the difficulties discussed earlier in this

chapter of choosing the most appropriate language and labels. Thus in one instance lavish graves are equated with paramount chiefs, rich with patrons and poor with poor clients (Higham *et al.* 2007, 649; see also Chapman and Gaydarska 2006), while in another account, involving complicated and somewhat abstract discussion of personhood, the lavish graves are seen as those of Melanesian-style ‘great men’; their appearance is argued to constitute a crisis of tensions between the ancestral egalitarianism of tell-dwelling communities and newly emergent patron-client relationships and conspicuous consumption, resulting in the burgeoning of the mortuary domain (J. Chapman *et al.* 2006, 165, 172–4; see also Chapman and Gaydarska 2015, 651–2). We do not seem to have any way to judge between alternative interpretations of this kind. Could we not equally summon up would-be hereditary aristocrats (Flannery and Marcus 2012, 197, 213), or transegalitarian despots or reciprocators rather than great men (Hayden 1995, 25)? Or should we go further, and propose something akin to the sacred kingship suggested for the claimed early big mounds in Brittany (Cassen *et al.* 2012; Pétrequin *et al.* 2012b; Jeunesse 2017a)? The argument there was based partly on the abundance of things, which some of the Varna graves amply display, and a passion for jadeitite and gold could be seen as part of a shared aesthetic of life-giving shine and glitter.

Careful ongoing analysis of the Varna goldwork has led to a more discriminating account of the varied depositions in the graves and a more cautious interpretation of the ‘complex social dynamics’ involved (Leusch *et al.* 2014, 165; 2015). That reemphasises the importance of the empty or ‘symbolic’ graves, and draws attention to the differences between objects such as pectorals, necklaces and tools/weapons, and between bodies and masks (Leusch *et al.* 2014, 165–8). This is reinforced by the range of gold-working skills on display, which are variously applied across the grave assemblages (set out in detail in Leusch *et al.* 2014; 2015). When the restricted number of the most abundantly furnished graves is put into context, and the empty graves are kept firmly in mind, the communal dimensions of supply, production and possession come to be emphasised again (Leusch *et al.* 2014, 168; 2015, 354–5). From that perspective, it is the existence of the cemetery as a whole that can be stressed, and as such it fits rather better into the wider context of mid- and later fifth-millennium cal BC south-east Europe (cf. Bartelheim and Krauss 2012; Windler *et al.* 2013).

The process of dating Varna has been instructive, and is incomplete. The first round was based on only 16 human samples. That suggested

that the richest graves could be early and short-lived, conforming to the picture often found of would-be elites or aggrandisers making their most extravagant displays early on in the process of their emergence and bid for power (Higham *et al.* 2007, 650–2). With a further 57 samples dated in a second round, and accepting the modest offset applied to the preferred model, graves containing gold are seen to occur through the probable two-century span of the cemetery as a whole (Higham *et al.* 2018). We should note, however, that it has remained difficult to date the empty graves, and correspondence analysis of the finds (Krauss *et al.* 2014) has still to be integrated with radiocarbon dating and modelling. Taken at face value, the current picture of the duration of the cemetery as a whole for up to eight generations does not easily square with the suggestion of crisis, though it could reflect endemic tensions and the playing out of processes of differentiation on the sort of timescales noted above in comparative studies. However these difficult issues are to be resolved, the further value of the Varna dating so far is its finite character. Whatever was going on was not prolonged into the later fifth millennium cal BC, and one study, of the Durankulak cemetery, suggests uncontainable tensions and internal collapse only at the end of the millennium (Windler *et al.* 2013).

The Varna cemetery appears to show the most pronounced differentiation in mortuary ritual of its times across south-east Europe as a whole. It serves to put the question, if differentiation and internal tensions might be the cause of the end of the tell system in the Vinča culture and roundabout, of what their scale might be. The answer, for both the northern and southern Balkans, may be that the extent of difference and potential competition does not need to have been extreme for profound change to occur.

Back to Alsónyék: the social conditions of aggregation and its decline

This brings us back to Alsónyék in southern Hungary, already discussed in [Chapter 4](#) to illustrate the possibility of constructing detailed and precise settlement histories. In short, the biggest site of its kind known in the Lengyel culture context of south-west Hungary was not at its maximum extent for very long, peaking in the generations around 4700 cal BC. That precedes the Varna cemetery further afield, and comfortably predates the end of the Vinča-Belo Brdo tell, but it appears to coincide with the end of the Uivar and Okolište tells, and with the beginning of the process of abandonment of other big Vinča sites through the 47th and 46th centuries cal BC (Borić 2015a; Whittle *et al.* 2016). The study of Alsónyék therefore has potentially much wider, comparative relevance.

One of the difficulties in understanding the Varna cemetery is the lack of the local settlement context. That is not the case at Alsónyék, since both houses and graves are co-present, and abundant. But in turn, putting Alsónyék into its local and regional context is at present a frustratingly incomplete challenge. Although in a particularising, historical approach I want to use the direct local and regional evidence for the case in hand, it is certainly instructive in this instance to take note of the very wide comparative literature on the phenomenon of settlement aggregation and coalescence. From a wide range of other times and places around the world, there are many examples of major and unusual co-residential aggregations of people, beyond the scale of normal villages, but without implications of urbanism. These do not appear to be the outcome of internal population growth, but of ‘processes of aggregation that, by and large, involved people abandoning a regional pattern of small, dispersed settlements in favour of aggregation into larger, more nucleated settlements’ (Birch 2013, 3). One review of ‘coalescent societies’ (Kowalewski 2006) has listed among other things movement to new locations and collective defence; the intensification of local production and trade; elaboration of community integration through corporate kin groups and moieties; unilineal descent groups or clan systems; architecture and layouts which promote integration; universalising, collective and egalitarian ideologies and ritual practices; myths which emphasise the incorporation and ordering of groups; and an emphasis on collective or corporate leadership and discouragement of hierarchy. To take just one specific example (others are discussed in Bánffy *et al.* 2016, 297–8), in the context of an increase in violent inter-communal conflict much bigger settlements with clustered longhouses and palisade defences emerged quite rapidly in the mid-15th century AD among the Iroquois, and the last of these had already been abandoned by about AD 1530 (Birch 2012; Birch and Williamson 2013, 158).

Now we know of other large Lengyel sites in the region of Alsónyék, including Mórág-Tűzkődomb, about 6 km away, and Zengővárkony and the site of Lengyel itself, both not much more than 20 km distant. The latter two especially are very large sites, best known for their burials, though timber-framed houses are known at Zengővárkony (Osztás *et al.* 2016b, 192), and exploratory trenching there suggests that it could be up to 40 ha in extent (Zalai-Gaál 2010). These sites have not yet been dated with the same precision as Alsónyék, though their material suggests broad contemporaneity (Zalai-Gaál *et al.* 2014), so we cannot yet tell whether these sites as a group represent some sort of sequence (as seen in the Iroquois study) or were local rivals, or

both.⁹ Plenty of other Lengyel sites, including substantial enclosures, are known in south-west Hungary or Transdanubia as a whole (Bertók and Gáti 2011), but it is not yet clear for the region around Alsónyék how all the evidence fits together and whether that great aggregation was preceded by a pattern of smaller and more dispersed sites. Some of the evidence from within Alsónyék could support the possibility that at its peak it stood as the principal place of residence for the local population as a whole. There is a high proportion of wild game, which constitutes roughly half of the faunal assemblages (Osztás *et al.* 2016b, 196); by analogy, that might reflect something like the kind of wide-ranging, seasonal deer drives seen in the Iroquois study (Birch and Williamson 2013).



Fig. 5.6: Lengyel burials at Alsónyék, in grave groups (clockwise from top left) 52, 76, 14 and 68. After Osztás *et al.* (2016b).

There is still a great deal of post-excavation analysis to be done on Alsónyék, so we must be cautious in what can be claimed at this stage of research, but first impressions of the site and careful study of other major nearby Lengyel concentrations of burials like Mórág and Zengővárkony (Zalai-Gaál 2010) do not suggest pronounced differentiation in mortuary ritual. At Alsónyék the strong impression is of a series of grave groups right across the different areas of the site (Osztás *et al.* 2016a, 15), which could well represent neighbourhood groupings of some kind, since they are interspersed with the distribution of houses and pit complexes. That could also be the case at Mórág and Zengővárkony and elsewhere. The Alsónyék grave groups vary in extent, order (tightness of the grouping, formality of rows and so on) and numbers, the smaller with some 25–30 graves, but the largest ones reaching over 100 (Osztás *et al.* 2016b), and there are also scattered graves seemingly more on their own, but on analysis so far no single grave group appears to stand out as dominant or preeminent. Likewise there are no obvious differences in the size or clustering of the Alsónyék houses (Osztás *et al.* 2016b), and no obvious central space has been identified. Men, women and children are all represented. The normal orientation of the burials was east–west, with the head to the east, but with faces to the south (at least in subsites 10B and 5603). The dead were predominantly laid to rest in a contracted position on their left side. Some of the burials were found in higher soil layers without any detectable grave cut. The most common graves were oval-shaped pits (Fig. 5.6), and there were some rounded rectangular pits.

That is not to claim that all burials – and it will be remembered that some 2300 were discovered in the excavated portion of the site – are identical. Plenty of these graves were unfurnished, but a typical grave assemblage has been characterised as having one to four pots and a stone or bone tool (Osztás *et al.* 2016b, 189). A number also have rather more abundant grave goods, including more pots, polished and chipped stone tools including axes, adzes and impressive flint blades, copper ornaments in the form of arm rings, finger rings and necklaces of beads, and beads in *Spondylus* and *Dentalium* shell (Osztás *et al.* 2016b, 189). Those kinds of assemblages seem to characterise a further distinctive grave type, rectangular pits with postholes in the four corners; these appear to be most numerous in subsite 10B, in the northern part of the site (Osztás *et al.* 2016b, 189). Two striking examples both came from grave group 23. Grave 3060 (Fig. 5.7) is the burial of a right-crouched adult male (aged 40–49 years) in a four-post grave pit, with six pots, three different types of polished stone axe, a

polished macehead, flint blades including one of the longest examples known so far from the Carpathian basin, a necklace of *Dentalium* and copper beads, and a very rich assemblage of *Spondylus* ornaments. There were also a large pair of aurochs horns, a fragment of an aurochs scapula, a polished red deer antler and a piece of bone from a griffon vulture. This burial is considered the richest male burial from this time period in eastern-central Europe (Zalai-Gaál 2011; Zalai-Gaál *et al.* 2012). The woman in grave 1473 had similarly abundant grave goods, especially of *Spondylus* and *Dentalium* (Bánffy *et al.* 2016, 308) (Fig. 5.7).

Further signs of difference come at the scale of the grave groups rather than in individual graves. Within subsite 10B in the northern part of the site, the body positions of the deceased in the adjacent grave groups 13, 14 and 15 all differ (Osztás *et al.* 2016b; Bánffy *et al.* 2016, 308). In grave groups 61 and 61A, layouts vary, with rows beginning to form in some places, but not in others. The grave groups appear also to have been in use for variable spans of time, as seen in grave groups 56–59 and 61–68 (Osztás *et al.* 2016b; Bánffy *et al.* 2016, 308).

Future analysis will undoubtedly tease out many more variations within Alsónyék. Numerous previous studies by the late István Zalai-Gaál have served also to show repeated variation in the abundance and character of grave goods in the major Lengyel cemeteries of the region (Zalai-Gaál 2010, figs 126–8). A principal conclusion was that the recurrent groupings of grave goods reflected some sort of hierarchical system, not just differentiating individuals or adult men, but family or other social groups, with some children marked by elaborate grave goods as well as adults; this was argued to represent an intensification of status differences compared with the LBK of the later sixth millennium cal BC (Zalai-Gaál 2010, 256). No specific labels or analogies were applied to this situation; preliminary papers on Alsónyék, however, refer in a summary kind of way to the occupant of grave 3060 as a *Häuptling* (chief or headman) (Zalai-Gaál 2011), and briefly to social hierarchy as seen in the male grave 5063/927 (Zalai-Gaál *et al.* 2012). On preliminary observations, it seems likely that the grave groups at Alsónyék could conform to the patterns outlined by previous studies. The chronology established for Alsónyék, moreover, strongly suggests a rapid history of aggregation and probably too of coalescence: the bringing together of previously separate communities. It is also not impossible that sites like Mórággy and Zengővárkony represent a similar process. If that is the case, there is no compelling argument for major individual social figures pulling

the strings behind the formation of sites like Alsónyék, Mórág and Zengővárkony, and every reason to seek the social dynamic – at one level – in the interplay of local community relations and politics, including perhaps at the scale of clans, lineages and families.

One of the difficulties with this view is that we do not so far understand the specific conditions and circumstances in which aggregation took place at Alsónyék and other sites. While comparative studies (e.g. Kowaleski 2006; Birch 2013) suggest the push of troubled times and violence as a recurrent spur to aggregation, there is so far no clear evidence for extensive trauma on the skeletons of people from Alsónyék or other major Lengyel sites in the region (Bánffy *et al.* 2016, 305–6), of the kind now widely recognised in other situations in the European Neolithic (Schulting and Fibiger 2012). Evidence of tuberculosis was found in grave group 13 in subsite 10B (Osztás *et al.* 2016b, 212, 215); this was also found at Hódmezővásárhely-Gorzsa in the south of the Great Hungarian Plain, from a broadly comparable date to Lengyel Alsónyék (Masson *et al.* 2015). While disease has been neglected in explanatory models, it is perhaps more likely that the incidence seen in grave group 13 was the outcome of close living, and potentially a reason for the breakdown of coalescence, rather than the cause of aggregation in the first place.



Fig. 5.7: Lengyel graves and grave goods at Alsónyék. Graves 3060 and 1473 are top and lower left, respectively. After Osztás *et al.* (2016b).

Our chronological models for Alsónyék have also suggested that the first Lengyel activity at the site took the form of burial, in subsites 5603 and 11 (Osztás *et al.* 2016b, fig. 25). So there may be a rival case for seeing locally and regionally preeminent lineages, led by prominent men and women, seeking to establish reputation through mortuary rituals and practices which emphasised small groups, descent and perhaps the inheritance of relative social position. Such a model might apply more widely, including to the tells and associated flat sites of the Hungarian Plain, as at Polgár-Csőszhalom (Raczky *et al.* 2015), and some such features would be one way of thinking of the conditions within the late Vinča complex, rather than the stark and somewhat context-less alternative of the autonomous individual household. The conditions in which aggregation as at Alsónyék took

place could also have been the cause of its decline. If competition brought people together, it also quite quickly drove them apart again. The site did not end instantly, however, and our models have charted a long decline. This may also speak to the circumstances in which tell settlements ended over a period of a couple of centuries.

The LBK: the social consequences of an accelerated history

There has long been a broad spectrum of opinion on the nature of social relations within the LBK (reviewed selectively in Whittle 2009, 253–4; see also Bickle and Whittle 2013, chapter 10; Hofmann 2012). Some have believed that these were basically egalitarian communities, as in Peter Bogucki's view (1988, 123) that network rather than hierarchy was the key in conditions of risk; in a rather earlier survey, I largely agreed (Whittle 1996). Others sensibly hedged their bets. Pieter Modderman (1988, 120–2) noted regional variation, the apparent dominance of older males in the Nitra cemetery in Slovakia and the possibility of more pronounced differentiation towards the end of the LBK sequence. In support he quoted studies by van de Velde (1979) and Milisauskas (1986), but for him the observation was significant that burials with varying amounts and kinds of grave goods were mingled together in cemeteries. Pieter van de Velde (1979; 1990) argued on the basis of both grave goods and the structure of ceramic decoration that Elsloo in the Netherlands showed a both patrilineal and matrilineal society, while Sarunas Milisauskas (1986) used the uneven distribution of stone axes and obsidian across the longhouses at Olszanica in southern Poland to suggest tentatively the presence of Big Men. Those possibilities were reinforced by further studies such as by Christian Jeunesse (1997) who found different traditions of mortuary practice, some assemblages including abundantly furnished child burials, suggesting pronounced differentiation and inherited social position.

Some of these positions have been restated more recently. The case for egalitarian LBK communities has been set out by Anick Coudart (2015), resting fundamentally on the homogeneity of longhouse architecture and the conditions of risk in which LBK people are seen to have operated. Crushingly conservative social norms were asserted by Ulrike Sommer (2001). I have noted above Brian Hayden's claim (1995, 41) that aggrandisers were already at work in transegalitarian LBK society, in the form of 'Despots'. Detlef Gronenborn and colleagues (Gronenborn 2016; Gronenborn *et al.* 2017) have also made the case for social figures akin to Melanesian Great Men, with an appetite for warring and the power to punish others. Subtler models have also been suggested, such as the view of a developing lifecourse

advocated by Daniela Hofmann (2009; see also D. Hofmann 2012; in prep.), according to which different kinds of male and female persona were projected through mortuary practice as life progressed, or the vision of small village communities with multiple elements in them (Gomart *et al.* 2015). That is based on study of Cuiry-lès-Chaudardes in northern France (see also Hachem 2011); if each longhouse was basically independent and autonomous, it was also set in a network of relations with other households, which were all potentially at different stages of economic growth or specialisation. I have referred earlier to the model of competing clans within Vaihingen in south-west Germany (Bogaard *et al.* 2011), and isotopic studies may suggest a widespread and recurrent pattern of dominant males, buried with stone adzes, with potential access to the best local soils (Bentley *et al.* 2012; Bickle and Whittle 2013, chapter 9).

So who is right, or are none or few of these models satisfying on their own? If the latter, that suggests that we have to seek more complex combinations. One way would be to assert the considerable diversity in LBK lifeways, seen generally across the spectrum of evidence for architecture and economic and mortuary practices, and reinforced by isotopic analysis of diet and lifetime residence and mobility (Bickle and Whittle 2013). That in turn allows there to have been different strands in LBK communities and networks. At some levels and in some places, the longhouse may have been independent, but there are many reasons for thinking that it was always also part of wider networks, which operated both at local and other scales. The sociality of houses set in rows (Rück 2009; 2012) was presumably rather different to those in more dispersed layouts (Jakucs *et al.* 2018), and there are long-recognised differences in dispersal and clustering across the landscape, whether or not some of the biggest sites are to be seen as ‘central places’ (Petrasch 2003); one study suggests large longhouses and enclosures as alternative means of social display (Pechtl 2009). Values of solidarity and communality may have been at odds with the opportunities to consolidate access to prime resources of land and soil (and thereby to improve cereal production and to increase the size of herds of cattle), and to assert principles of seniority and leadership through mortuary practice and exchange. In this putative mix of worldviews may have resided the dynamic of LBK social development, especially if dominant lineages or clans were already responsible for leading LBK colonisation at the start of the sequence (Friedrich 2005).

How then does robust chronology contribute? I believe that modelling carried out in the ToTL project helps in four principal ways

– though a lot more analysis is needed in future research to support and refine my suggestions. First, our Lower Alsace study (Denaire *et al.* 2017; detailed in [Chapter 4](#)) confirms the picture of local conditions of settlement which altered through time, which study after study have already suggested, but adds a tighter, generational timescale; in this perspective, nothing seems to have stayed static for long.¹⁰ Secondly, our limited modelling of individual sites (and here particularly there is enormous scope for further work) suggests significant variation in settlement durations, from probably nearly two centuries at Szederkény (Jakucs *et al.* 2016) and the same or longer at Alsónyék (Oross 2016b) to perhaps as little as a couple of generations at Versend (Jakucs *et al.* 2018) (see again [Figs 4.7, 4.9–4.10](#) and [4.13](#)). The layout of longhouses at Versend was clearly in well-organised rows, and although that might suggest pronounced solidarity, it could also have prompted many tensions and an early break-up of the site (though of course any number of other, contingent factors could also have been responsible for that). Other nucleated sites such as Vaihingen and Cuiry managed to persist for a while. So we should refocus on the dynamic temporality of LBK settlement (see also Hofmann 2012), rather than rely on a static, general picture of settlement of the *loess* and other good soils.

Thirdly, our modelling suggests a more compressed or accelerated LBK history than has been conventionally understood. Over and over again, the literature has suggested a start date for the LBK around 5500 cal BC, and in some accounts at earlier dates still, at 5600 or even 5700 cal BC (for example, Stadler and Kotova 2010). Our ToTL modelling suggests, provisionally (since the quality and number of samples can undoubtedly be improved upon in future research), a rather different picture. It is only the so-called Formative phase of the LBK, found in western Hungary at Szentgyörgyvölgy-Pityerdomb (Bánffy 2004) and in eastern Austria at Brunn 2a, outside Vienna (Stadler and Kotova 2010), that probably dates to that early point (Jakucs *et al.* 2016). The more widely distributed *älteste* or earliest LBK pottery style is then modelled as appearing, rather rapidly, to the east, north and west, in the middle of the 54th century cal BC, taking perhaps only a generation or two to spread over 1000 km (Jakucs *et al.* 2016, 323) ([Fig. 5.8](#)). It is not yet possible to define the end of the earliest LBK, but there are strong hints that in some situations at least this may have fallen later than the date of c. 5300 cal BC which is conventionally assigned to it. But from our Lower Alsace study, the models suggest the appearance there of the LBK, with LBK IIb or Flomborn style pottery, over the course of a few generations in the

first half of the 53rd century cal BC (Denaire *et al.* 2017, figs 8–9).

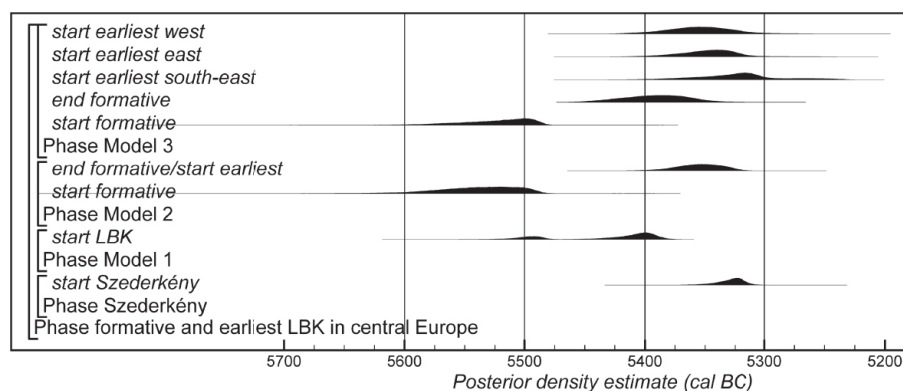


Fig. 5.8: Model for the LBK diaspora. After Jakucs *et al.* (2016).

This is not in fact far from the conventional estimate, but it is slightly later, and overall the picture emerging from formal modelling suggests a later and more accelerated start to LBK development than previously envisaged and therefore a less leisurely LBK history. In that perspective, though this has to be worked through in detail in any number of local and regional studies, there is the potential for even more people (compare population figures in Zimmermann *et al.* 2009) in the chosen LBK landscapes of valley edges and good soils, over the course of the 53rd–early 50th centuries cal BC. From this admittedly general assertion, we could envisage the conditions for more intense competition and jostling over prime resources and for the emergence from time to time and from place to place of dominant communities or factions such as clans or lineages within or cross-cutting particular communities.

Fourthly, and perhaps closely connected to a shorter duration for the LBK as a whole, our modelling has thrown up the probability of significant hiatus at the end of the sequence, as already set out in [Chapter 4](#). This contrasts sharply with previous expectations and observations of continuity between the LBK and succeeding groups and pottery styles, such as the Hinkelstein and *Stichbandkeramik* styles in the Rhineland and central Europe. In Lower Alsace, this hiatus may have lasted well over two centuries – from towards the end of the 51st century cal BC to the first half of the 48th century cal BC – though probably for a shorter period in other parts of the Rhineland (Denaire *et al.* 2017, figs 23–5). Whether the same situation might apply to the west, in the Paris basin, requires further research. On present indications there may be more continuity between the LBK and

Lengyel cultures in western Hungary; the steady development of the Alsónyék settlement (Bánffy *et al.* 2016; Osztás *et al.* 2016a) is one indication of this, though this too deserves much further research.

This unexpected picture puts into sharper relief the available evidence and models for conditions in the late LBK. There are the well known sites with killings, on varying scales, such as Talheim, Asparn-Schletz and Kilianstädten (Wahl and König 1987; Teschler-Nicola 2012; Meyer *et al.* 2014; 2015).¹¹ Some of the extensive deposits of intensely treated and disassembled human remains at Herxheim might be the result of killings too – before processes of dismemberment and even cannibalism – though the direct evidence for this is both slight and disputed (Orschiedt and Haidle 2012; Boulestin and Coupey 2015; Zeeb-Lanz 2016). The distribution of these sites roughly overlaps with those areas where we have modelled post-LBK hiatus. It overlaps, though not neatly, with the distribution of burials in Tradition II, which include signs of inherited social position (Jeunesse 1997; 2009); intensifying social difference in the late LBK has been mooted as part of the context for Herxheim (Boulestin and Coupey 2015). It remains to be seen in more detail how all these instances of sites with evidence for group-scale violence are to be dated; a more precise chronology badly needs to be established. Just where Herxheim falls needs further dating and modelling (Zeeb-Lanz 2016). Others of these sites could be slightly earlier (Meyer *et al.* 2014).

The difference to be established by further refinement of these chronologies is between a final, single, short-lived episode of intercommunal violence and disturbance, and a more prolonged phase of potentially very unsettled times. Climate-induced change is one possibility (Gronenborn *et al.* 2014) but is so far poorly dated. Modelling of population and other factors allows for a single period of famine to have caused major disruption of the late LBK (Bocquet-Appel *et al.* 2014). This is by no means impossible, though a longer chronology for troubles, if established, would argue against it (quite apart from the breadth and resilience of LBK economic practices), and it is not clear how one episode of famine could have produced such apparently widespread and prolonged hiatus subsequently. In our ToTL paper on Lower Alsace, therefore, we speculated whether epidemic disease of some kind could have been the principal agent of change, disrupting and ultimately destroying LBK settlement over wide areas and for significant periods of time (Denaire *et al.* 2017).

What kind of social label we should best apply to this situation remains open, since suggestions so far of Great Men or Big Men for example do not seem quite to catch the communal dimensions of what

was going on, nor the potential for resistance to aggrandisers through the assertion of shared values; many of the clues to difference are found in the mortuary sphere, where claims to superiority or seniority could perhaps more safely be asserted without open contestation. But the combination of the accelerated history of the LBK overall, the evidence for severe disruption in its later stages, and the models for significant post-LBK hiatus makes me much more open than previously to the possibility of more rather than less differentiation within LBK communities by the end, though probably playing out in the shared dimensions of clan and lineage rather than among individuals, and competition between communities is not to be ignored.

Southern Britain in the first half of the fourth millennium cal BC

I have already briefly sketched in [Chapter 1](#) the outline of major developments in monument construction in southern Britain in the first half of the fourth millennium cal BC. The outcome of programmes of formal modelling (Bayliss and Whittle 2007; Whittle *et al.* 2011) has been the construction of provisional but much more precise chronologies for the appearance and duration of long barrows and causewayed enclosures, along with their accompanying material culture, and also provisionally, for the process of the introduction of new things and practices, as we put it in *Gathering Time*, at the start of the sequence. That helped to give context to what now appeared as a much more dynamic sequence. In place of previously generalised accounts of the early Neolithic in southern Britain, with everything more or less lumped together in a period rather raggedly defined as lasting several centuries, comes a history of introduction and transformation – initiated in our view by immigrant communities in south-east England in the 41st century cal BC but spreading subsequently over a longer timespan than previously envisaged and with probable interaction with indigenous communities – followed by the gradual appearance, by around 3800 cal BC, of first, long barrows and long cairns, and then in turn the first causewayed enclosures, probably in south-east England, from the late 38th century cal BC. The 37th century cal BC emerges as a time of great activity, up until its last part when the pace appears to have slackened temporarily. People were progressively busy across the landscape of southern Britain with building barrows and enclosures; in a follow-up paper we have specified, for example, how many signs of recent or current building activity would probably have been visible to someone traversing from the upper Thames valley down to the chalk of Cranborne Chase in Dorset in the 3630s cal BC (Bayliss and Whittle in press). By the

mid-36th century cal BC the last new constructions of causewayed enclosures were undertaken and the first cursus monuments were probably constructed (though much more work remains to be done to pin down their chronology); some existing causewayed enclosures remained in use, just as there were late constructions of long barrows.

So far this picture appears robust, though fresh research is likely, as always, to lead to modification and refinement. The pattern for the development of enclosures was based on a substantial sample of dated examples; new discoveries, for example at Larkhill near Stonehenge and in the Thame valley (Alistair Barclay and Lisa Brown, *pers. comm.*) appear to conform to the enclosure chronology proposed in *Gathering Time*. The dating of Magheraboy, much further afield in western Ireland, remains challenging and problematic, but a very early date for it still stands out from the pattern of development in Ireland in the first part of the fourth millennium cal BC (Whitehouse *et al.* 2014). The chronology of long barrows and cairns is based on a much smaller sample of dated examples, and may be open to more change; the probably early date for the construction of the stone chamber at Coldrum in Kent (Wysocki *et al.* 2013) should be noted. Modelling of new dates for the construction and use of Wor Barrow in Cranborne Chase, in the 37th century cal BC (Allen *et al.* 2016), provides one more example of a construction that neatly fits the proposed overall picture. The debate about beginnings at the start of the sequence will no doubt rumble on; suffice it to assert that the pattern emerging from further research on both western Britain (Garrow *et al.* 2017) and Orkney (Griffiths 2016), as well as on Ireland noted above, makes the claims for very early – that is to say late fifth millennium cal BC – migration up into western Scotland, for example at Achnacreebeag, Argyll (Sheridan 2003b; 2010; 2016), implausible.

Given the scenario which I have outlined, what does a more precise chronology contribute to our understanding of social relations and social change? Previous social models for southern Britain have tended to focus on the late Neolithic, with chiefdoms (Renfrew 1973c) or ritual authority structures (Thorpe and Richards 1984) to the fore; the earlier part of the Neolithic has been comparatively neglected in terms of specific social modelling, causewayed enclosures merely evoking rather ill-defined ‘protochiefdoms’ for example (Renfrew 1973c), or major cairns (to jump north to Orkney) ‘segmentary societies’ (Renfrew 1979). With the substantial increase in the number of available analogies, which I have stressed earlier in this chapter, how could we read barrows and enclosures? There has been a tendency to take such constructions on their own, but out of context

and out of sequence. There are competing interpretations. On the one hand, the contents of long barrows and cairns could be seen as the remains of basically undifferentiated communities, lineages or families, and enclosures as the coming together of dispersed but essentially balanced communities. That would be to argue for a more or less egalitarian society through the early part of the southern British Neolithic, a view I had previously supported (Whittle 1996; Whittle *et al.* 1999; Benson and Whittle 2007). On the other hand, barrows and cairns could be seen to project the self-proclaimed importance of particular descent groups (probably with a much greater variety of forms of descent than we have allowed for), making claims to prominence, seniority and perhaps – from the repeated presence of children and juveniles – to inherited social position, as well as to land and resources. Likewise, causewayed enclosures, with their links to related practices on the adjacent continent, and the charisma, influence, renown and powers of persuasion surely needed both to mobilise and organise labour for their construction, and to attract the kind of participants in feasts and other activities required to validate all the effort involved (Whittle 2013), could be seen as a progressively wider attempt to exert social control, through powerful combinations of links to the past, innovative ritual, audacious labour enterprises and enticing, irresistible social gatherings (Fig. 5.9).

I believe that the more precise chronology now available tips interpretation in favour of more rather than less differentiation. After quite slow beginnings, by the turn of the 39th century cal BC, particular corporate groups, probably varied descent groups, began to claim position and place with the construction of barrows, using their dead as markers of seniority and solidarity. The tempo of change quickened further by the turn of the 38th century cal BC, with enclosures, an old idea in terms of continental practice, being introduced as a striking, perhaps daring, innovation in the regional context of southern Britain, and perhaps brought in through old links going back to initial colonisation, which would have served again to assert principles of seniority. The first 75 years or so of the 37th century cal BC in particular emerge as a time of much activity, and vying for prominence. There is evidence for individual killings, and for the burning of parts of enclosure circuits (Benson and Whittle 2007; Whittle *et al.* 2011, 896–7), so that violent rivalries and jealousies seem a real part of the proposed competitive scene, though it seems unlikely that they characterise all social interaction in these generations. They may speak in part, however, for resistance to aggrandisement, and the brief lives of some enclosures or enclosure

circuits may indicate accelerated cycling in the fortunes of groups and communities.

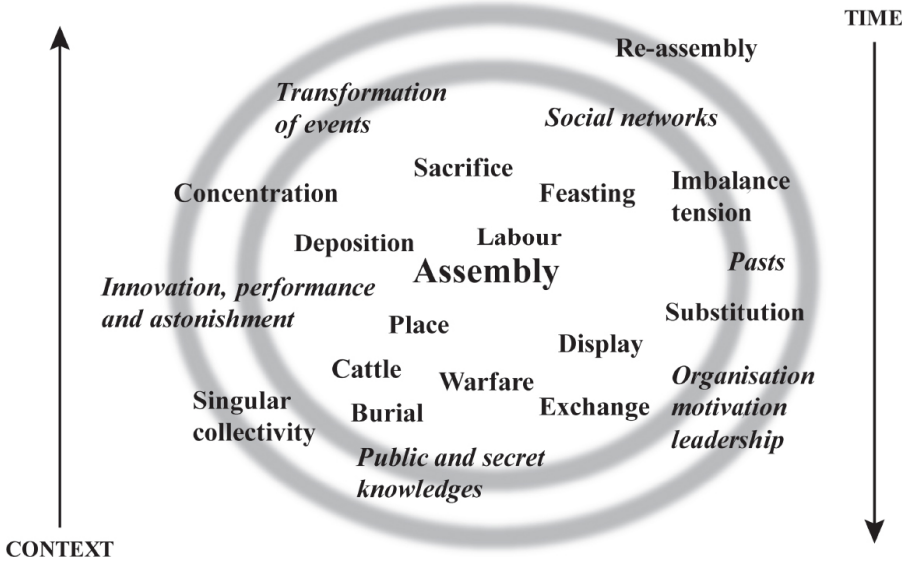


Fig. 5.9: A model for activity and temporality at ditched enclosures. After Whittle (2014).

We lack the detailed evidence for much of southern Britain for what was going on in the wider landscape with land, clearance, cultivation and the size of the cattle herds, from which animals were chosen for consumption in enclosure gatherings (cf. Parmenter *et al.* 2015); in ToTL modelling of pollen sequences from the Somerset Levels, both 3800–3600 and 3600–3400 cal BC stand out as periods of increased landscape openness (Farrell *et al.* submitted). There would presumably have been the option of dispersal in the face of threats of control and violence, and the means of self-defence were freely available. My vision of large enclosures in action is one of noisy, rather chaotic scenes, which would have been hard to control: another form of resistance to social dominance. It may be, if a trend to social differentiation continued in linear fashion through the middle of the fourth millennium cal BC, that it was no accident that enclosures came to be replaced by the more arcane and mysterious presence of cursus monuments, the larger of which required much labour as well as access to land, but in which movement and ritualised action could be more easily controlled (Fig. 5.10).



Fig. 5.10: History in the making: a cursus monument overlying two probable phases of causewayed enclosure at Fornham All Saints, Suffolk. After Whittle et al. (2011).

What labels, finally, are to be put on this shifting, dynamic sequence is an open question. In line with the argument of this chapter, this study in particular indicates the difficulties of plucking rather static and fixed labels from the comparative, evolutionary literature, and applying them to the narrative of a rapidly changing sequence. If the argument for an increasing tempo of change through the first centuries of the fourth millennium cal BC is accepted, much depends on how we characterise the start of things. Begin with more or less egalitarian colonising communities, and we perhaps end up with Big Men, aristocrats or some kind of local chiefs dreaming up and directing the building of cursus monuments; start with transegalitarian colonisers (as suggested for the LBK: Frirdich 2005; see also D. Hofmann 2015 on the agency of colonisers) and we could perhaps already see a bigger range of kinds and types of aggrandisers or aggrandising social groups behind the construction of barrows and

cairns, whose interaction and rivalry then drives the pace and somewhat frenetic character of construction through the 37th century and beyond.

Northern France in the second half of the fourth millennium cal BC: readings of the collective dead

My account of the southern British sequence implies efforts to assert social position and control of ritual innovation and practice which steadily increased through time. A similar sort of development was proposed in the study of the Mittelbe-Saale region (Müller 2001; see above). The situation in northern France in the second half of the fourth millennium cal BC, however, may subvert any temptation to see neat, linear evolution through time. There are also strong echoes in other sequences at this time.

This takes us back to the *allées sépulcrales* and *hypogées* of the Paris basin, already briefly discussed in [Chapter 4](#). Together, these present a strongly collective (though far from uniform) burial practice. The *allées sépulcrales* are very similar to the so-called gallery graves in Hesse, and not dissimilar to later TRB monuments including the *Totenhäuser* of the Mittelbe-Saale region. The passage graves in Denmark and southern Sweden typically also contain collective deposits of human remains. While the architectural styles of these various monuments differ, they appear to be united by some sort of shared concern to gather up large deposits of human remains; they also appear to be broadly contemporary (Müller 2001; Schulz Paulsson 2010; 2017; Schierhold 2014; Hofmann and Orschiedt 2015, 998–99; Sjögren 2015, 1007). Hypogea and cave deposits in southern France probably also belong to this horizon (Guilaine 2015), and in turn these have links to similar practices in the central and western Mediterranean and Iberia (Robb 2007; Balsera Nieto *et al.* 2015).

There are many of these sorts of monuments, though with varying densities and kinds of distributions. At a rather general level, the numbers of dead assembled over a potentially short span of generations could be seen as a celebration and renewal of the fertility and vitality of the community, both essential for the tenure of place, and conversely as a counter to the fear of dispersal of the assets of the community, which are recurrent features of funerary rites in many cultures observed by anthropologists (Bloch and Parry 1982, 18–21; Bloch 1982, 212–13, 218–19; Metcalf and Huntingdon 1991, 108). In Les Mournouards, virtually the last body to be placed, in the centre of the main chamber, was a heavily pregnant woman (Chambon *et al.* 2018) ([Fig. 5.11](#)). As another general assertion, perhaps there were increasing concerns in the Paris basin and widely elsewhere in the

later fourth millennium cal BC about land and numbers of people. It has been argued, on the basis of pollen diagrams and monument trajectories, that the second half of the fourth millennium cal BC in northern Germany was a time of agricultural intensification (Hinz *et al.* 2012; see also Pétrequin 1997 on eastern France), and perhaps something of the same applied in the Paris basin and elsewhere at this time. Now it was commonplace in processual archaeology to apply generalised ideas such as the Saxe-Goldstein Hypothesis 8, about the links between formal disposal areas, scarce or vital resources and the existence of corporate descent groups (reviewed in Morris 1991), or the notion of territorial markers (Renfrew 1979). Perhaps something of both ideas could be applied to the situation of the varied collective deposits of the later fourth millennium cal BC. The large numbers of tombs with varying but probably corporate groups buried in them could speak strongly to concerns over resources and land.

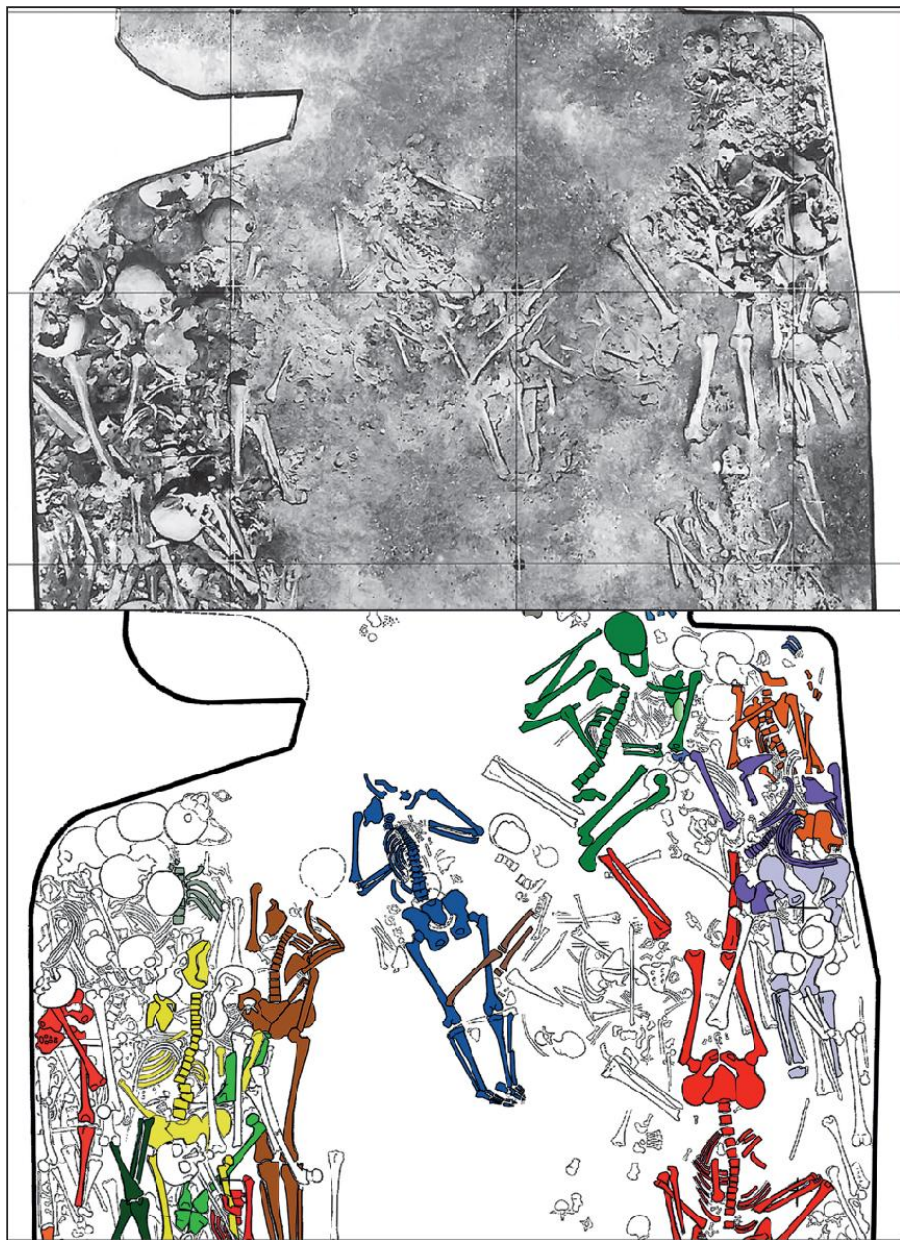


Fig. 5.11: Detail of the final deposits in the centre of the Les Mournouards hypogée. After Chambon et al. (2018).

But what sort of corporate groups? It is tempting to combine the size of the deposits and the apparently short periods of time over which the deceased were entombed, and to see a kind of ‘triumph of the commons’, with access to the monuments open to large numbers of men, women and children. On that basis, this could be read as a less rather than more differentiated social scenario. Further in support

of this view, at least for the sequence in northern France, would be the changed character of mortuary practice in the reuse of the *allées sépulcrales* in the *Néolithique final* of the earlier third millennium cal BC (the *hypogées* hardly ever have evidence of reuse at that time), with smaller and more compartmentalised deposits in monuments like Bury and La Chaussée-Tirancourt (Salanova *et al.* 2017; Leclerc and Masset 2006), which could be read as the expression of more tightly defined descent groups. This was also again the time of large houses, which could also project social prominence and control (Tinévez 2004; Vander Linden and Salanova 2004).

It is important to stress that in the *Néolithique récent* the *t allées sépulcrales* and the *hypogées* were far from identical, the former more dispersed and visible, being only partly sunk into the ground, the latter tending to form cemeteries – given the timescales, with at least some constructions being successive – and concealed, being firmly set underground. Artefacts accompanying the dead may pick out more internal groupings within the *hypogées* than in the *allées sépulcrales* (Blin 2015; Chambon *et al.* 2018). Though there is a lot more research to be done with isotopic and aDNA analysis, it could be that many *allées sépulcrales* served principally local communities, while the clusters of *hypogées*, confined to the Marne region, could have been used by wider populations in the region (Chambon *et al.* 2018). Once again there is no easy label to apply to this situation from the comparative literature. Could these and other comparable monuments express conflicting or competing social values? On the one hand they could have projected a strong sense of communality and solidarity, while on the other accommodating principally the remains of selective strands or groupings within society – for example, families, lineages or clans – assiduously assembled in times of pressure on land and territory.

Valencina de la Concepción: extravagant funerals in the early third millennium cal BC

We have already seen the complex of Valencina de la Concepción in south-west Spain in [Chapters 1](#) and [4](#). Pre-Beaker Copper Age activity there lasted from the 32nd to the 25th century cal BC (García Sanjuán *et al.* 2017; 2018). Within this span, a more nuanced narrative emerges from the modelling of a series of funerary contexts. Artificial caves or deep pits were used for successive inhumations over quite long periods of time, as at La Huera, Calle Dinamarca and IES (García Sanjuán *et al.* 2018, figs 54–5). La Huera was in use for well over a century. The main focus of the mortuary rite was the successive deposition of intact corpses. The relatively scarce grave goods were

not of particularly unusual character. Skeletal remains were moved or otherwise disturbed by succeeding depositions. This and other similar deposits might be seen as the focus of a small but persistent group, such as a family, lineage or other kin-based grouping, or some kind of co-resident neighbourhood. From the 29th century cal BC, and lasting to the middle of the third millennium, a diverse range of collective burial in simple pits was also evident (García Sanjuán *et al.* 2018, figs 54–5), found in various sectors of the complex, including PP4-Montelirio, La Alcazaba, La Cima and perhaps El Algarrobilllo. None of the pits appear to be as large as the ‘artificial cave’ of La Huera. There was a spectrum of kinds of deposition, from articulated or probably originally articulated corpses, to partial remains, possibly including crania on their own. The quantities of human remains vary, as well as the abundance of other material.

In this sequence, the first set of tholoi appeared at Valencina in the 29th century cal BC (though they may have been in use elsewhere in southern Iberia at a slightly earlier date: Balsera Nieto *et al.* 2015) and do not appear to have lasted beyond the 28th century cal BC (though the sample is admittedly small and the material amenable to dating of variable quality) (García Sanjuán *et al.* 2018, figs 54–5) (Figs 5.12–5.18). These tholoi, perhaps largely mud-vaulted, offer a more defined and formal, if simple, architecture, with a three-dimensional, above-ground, circular mound, a passage and a chamber, generally circular. Quite modest, small examples of this kind exist in quantity, for example in the PP4-Montelirio sector (García Sanjuán *et al.* 2013a). But a few were much larger, taking prominent positions among the less spectacular structures which seem to crowd around them (Fernández Flores and García Sanjuán 2016, fig. 2; Mora Molina *et al.* 2013, fig. 2). The 70-m diameter Montelirio tholos, already described, is one example (Fernández Flores *et al.* 2016), and not far away, Structure 10.042–10.049 of PP4-Montelirio is another example; excavation has shown a more or less circular, blank area surrounding the long corridor and paired chambers, presumably (though speculatively) indicating a substantial mound with a diameter potentially of up to 40 m or so (Fig. 5.12). The architecture of these bigger structures was not obviously different in basic design, but as well as their size, these stand out for the elaboration of chambers with carefully built stone-slab walls, in the case of Montelirio further enhanced in the bigger chamber by engraved and painted motifs (Bueno Ramírez *et al.* 2016), and the elongation of slab-lined and covered passages: narrow, low and presumably extremely difficult to access (Fig. 5.13).



Fig. 5.12: View over the PP4-Montelirio sector at Valencina de la Concepción, with structure 10.042–10.049 in the foreground, and the Montelirio sector in the background. Photo: Leonardo García Sanjuán.

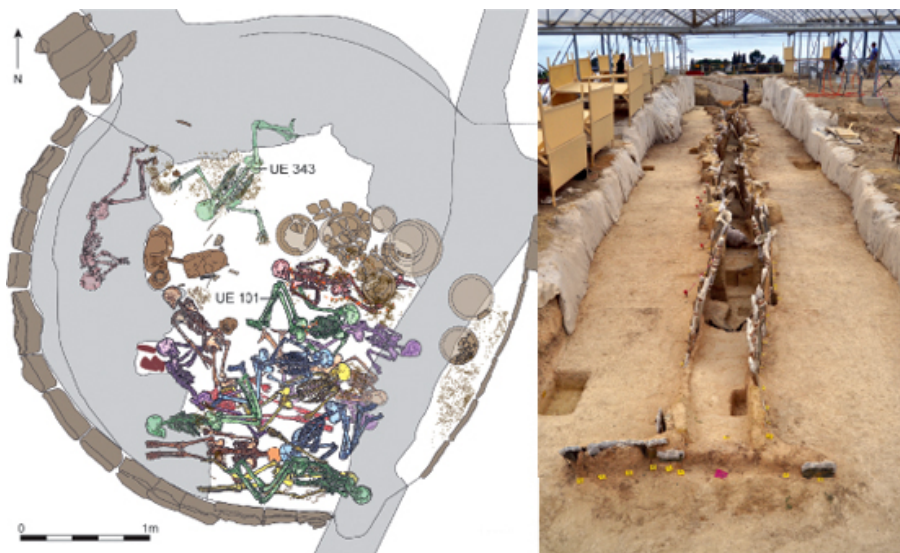


Fig. 5.13: The Montelirio tholos. Left: chamber plan (with positions of UE 343 and UE 101 only marked); right: the passage. After Fernández Flores et al. (2016).

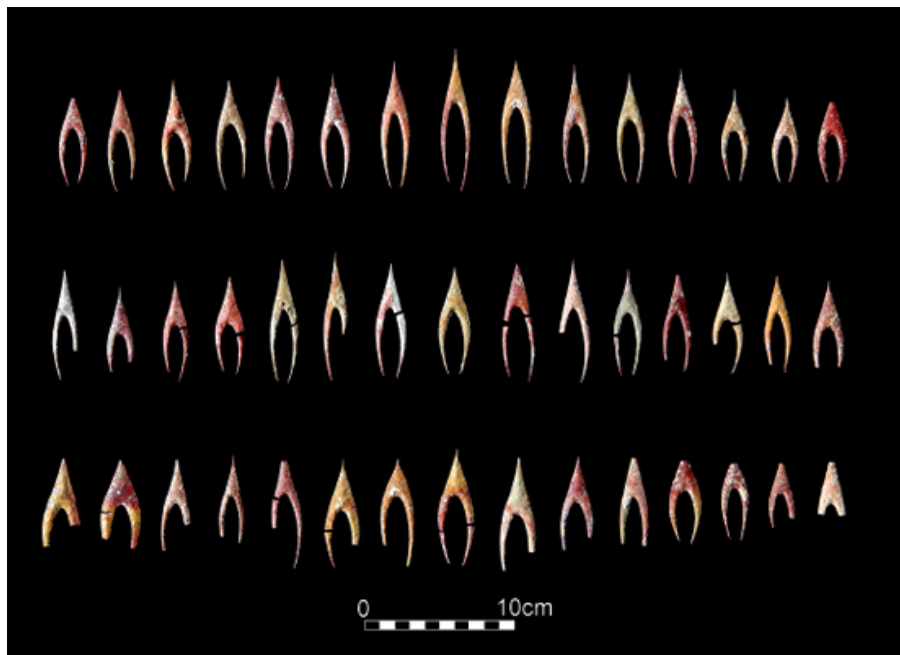


Fig. 5.14: High-quality flint arrowheads from the chamber of the Montelirio tholos. After Fernández Flores *et al.* (2016).

These impressive constructions were further distinguished by their contents (Figs 5.14–5.15, and 5.17–5.18). The human remains found in them varied. In the lower level of PP4-Montelirio Structure 10.049, as already described, there were the remains of a single adult male; it is not known, because of later disturbance, what the contents of the paired, slightly smaller chamber, Structure 10.042, may have been, but there were four further individuals at the end of the passage. In the main chamber of the Montelirio tholos was a collective deposit of some 20 people, dominated by the presence of adult women (Fernández Flores *et al.* 2016). The 10.049 individual is distinguished by the grave goods directly accompanying him and in the deposit above him, but it is not known how, or if, he was clothed; none of these finds are obvious ornaments or items of dress. In the Montelirio main chamber, however, the importance of the context is enhanced not only by a decorated stela in the floor and abundant deposits of red cinnabar, including on the dead themselves, but also by the costumes which many of the deceased were either wearing or were covered by (Díaz-Guardamino Uribe *et al.* 2016) (see again Figs 1.5–1.6).

The evidence for such funerary apparel consisted of concentrations of small perforated beads, many lying in identifiable lines, presumably sown together or onto a perishable backing, forming cloaks or heavy dresses. The great majority are made of some kind of marine bivalve

shell, though there are also amber beads. A total of a million beads has been estimated, whose manufacture must have consumed prodigious hours of labour. There are also fragments of gold (Murillo-Barroso 2016), and small objects in ivory including what seem to be representations of acorns and pigs (Luciañez Triviño and García Sanjuán 2016) might also have been personal ornaments (Fig. 5.15). One of the women (UE 343) in the main chamber of Montelirio was deposited with outstretched arms (García Sanjuán *et al.* 2016), which might be interpreted as a possible representation of dancing or shamanic trance (Fig. 5.16); another (UE 101) had an extra toe (Pecero Espín 2016, 436), another potential marker of special difference. We do not have any detail from the second chamber in Montelirio, because of the conditions of preservation, though fragmentary human bone and ivory objects were recovered.

As already sketched in earlier chapters, there were many other skilfully made and exotic things in Montelirio and PP4-Montelirio 10.042–10.049, for example in ivory, gold, flint, rock crystal and amber (Figs 5.14–15, and 5.18). These came from considerable distances: ivory from Africa and ‘Asia’ (presumably the Near East) (Schuhmacher *et al.* 2013; García Sanjuán *et al.* 2013b); gold from perhaps two alluvial (probably more local) sources (Murillo-Barroso 2016; Murillo-Barroso *et al.* 2015); flint from the Baetic mountain range of south-east Spain (Murillo Díaz 2013); rock crystal probably from somewhere in central or central/south-east Spain (Morgado *et al.* 2016, 12–13); and amber probably from Sicily, on the basis of spectrographic analysis, though there are deposits in Iberia itself (Murillo-Barroso and Martín-Torres 2012; Murillo-Barroso and García Sanjuán 2013). Superb craft skill is seen mainly in transformative processes: the remarkable flaking of the flint arrowheads from Montelirio and the rock crystal blade from PP4-Montelirio 10.049, the exquisite carving of ivory in these and other contexts, or the beating out and decoration of gold foil. Exotic material was also acquired in an unaltered state, as in the ivory tusk and ostrich egg found in PP4-Montelirio 10.049 (García Sanjuán *et al.* 2013a) or the cinnabar (mercury oxide) used so liberally in Montelirio (Rogerio-Candelera *et al.* 2013). The cinnabar probably comes from central Spain, and the ostrich egg from north Africa. It is worth noting that the combination of African and Asian ivory was found only in PP4-Montelirio 10.049; other contexts in Valencina have one or the other, and the site appears to sit at the overlap between two complementary distributions in southern Iberia (García Sanjuán *et al.* 2013a, 626, fig. 14; see also Valera 2017, fig. 9). Also striking is the

absence of copper from these two prominent tholoi (García Sanjuán *et al.* 2016); gold too is not confined to such contexts, as witnessed in the large gold plaque found in PP4-Montelirio Structure 10.029, a simple pit with no human remains, such that different kinds of possession and value can be entertained (Murillo-Barroso *et al.* 2015: a reflection worth taking back to Varna, as discussed earlier in this chapter).



Fig. 5.15: Ivory pigs and acorns from the chamber of the Montelirio tholos. After Fernández Flores *et al.* (2016).



Fig. 5.16: Reconstructed female figure from the chamber of the Montelirio tholos. After Fernández Flores et al. (2016).

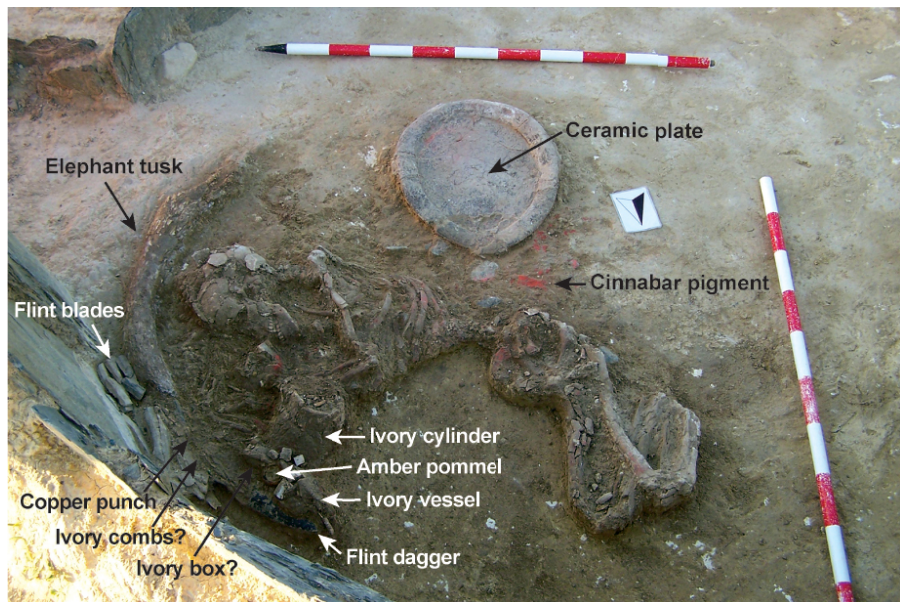


Fig. 5.17: The lower level of structure 10.049 in the PP4-Montelirio sector. Photo: Leonardo García Sanjuán.

Even this brief review underlines the exceptional character of these contexts. Set within a more robust (though still far from perfect) narrative of development at Valencina, and with a *floruit* of probably only a few generations, the special nature of these tholoi is further enhanced. A new architecture of separation and display is adopted, for which labour had to be mobilised. The funerals concerned could altogether have been much more showy than the depositions in for example the artificial cave of La Huera; the burials in Montelirio could represent a single event (see longer discussion in Fernández Flores *et al.* 2016; García Sanjuán *et al.* 2018), as was presumably the case for the lower deposit at least at PP4-Montelirio 10.049. The grave goods assembled were exotic, not to say extravagant, showing not only high craft skill but perhaps also life force and the power of the distant (cf. Helms 1988; 1998).

It has even been suggested that the burial in PP4-Montelirio 10.049 resembles Corded Ware practice – hundreds if not thousands of miles away in central and eastern Europe – since it has a right-sided crouched body position, oriented east–west, and a flint dagger (Heyd 2017, fig. 3); that is backed up by the assertion that the possibly sandal-like representations in southern Iberia, including in ivory from the upper deposit in 10.049, and in gold from the separate pit Structure 10.029, in PP4-Montelirio, somehow match representations of paired feet on Yamnaya/kurgan stelae in Ukraine (Heyd 2017, 354, fig. 4). Such far-flung connections do not seem impossible to me,

though they are very far from certain, but we cannot be certain about the chronology; the 10.049 burial could predate even the earliest Corded Ware horizon in Europe (if that is not before 2800 BC: see Włodarczak 2009, and [Chapter 6](#)), and there are plenty of crouched burials in other Valencina contexts. Nor are the Ukraine stelae precisely dated (Volker Heyd, *pers. comm.*). In addition, the ivory ‘sandal’ has been reconstructed as part of a plaque or even a sheath for the rock crystal dagger (García Sanjuán *et al.* 2013; *contra* Heyd 2017, 354). Even without this arguable connection far to the northeast, the connectivity seen at Valencina in the tholos phase is impressive enough. Finally, these funerals may present an interesting twist on established traditions of descent at Valencina. They are set amongst other burials, and presumably respect those; La Huera is a little further off to the east. At the same time, they distance themselves in the ways described from the spectrum of other mortuary practice at Valencina. In that perspective, it would be important to somehow establish in future research the relative order of Montelirio and PP4-Montelirio 10.049. They are only 100 m or so apart. Do they represent successive generations, or rival descent groups?



Fig. 5.18: High-quality grave goods from structure 10.049 in the PP4-Montelirio sector. Top: ivory tusk from the lower level; middle: decorated ivory tusk from the upper level; below left: flint blade from the lower level; lower right: reconstructed dagger with rock crystal blade and ivory pommel and sheath, from the upper level. Photos: Leonardo García Sanjuán.

If, as discussed earlier in this chapter, the hypothesis of some kind

of early state formation or polity centred on Valencina seems anachronistic, can we go further than the rather general interpretations of a ‘social elite’ (García Sanjuán *et al.* 2013, 629) or of prominent ‘kin groups, factions or individuals’ with access to special materials and artefacts (Morgado *et al.* 2016, 245), echoing the competitive, ranked kin groups suggested for Los Millares (R. Chapman 2008, 203–4)? Interpretation of the Montelirio burials in their own terms has noted the overall grandiosity of the context but also the lack of specific associations between individuals and outstanding grave goods (García Sanjuán *et al.* 2016, 542–3), and has suggested that this group could have been some kind of religious or mystical specialists, with an elevated, elite social position (García Sanjuán *et al.* 2016, 544–7). Neat labels are deliberately avoided. But in this instance, the similarities in many aspects to the pretenders to chiefly power and inherited social position, as set out earlier in this chapter, are very striking. Though I have criticised the implied evolutionism of the treatment of Flannery and Marcus (2012), their descriptions (especially in their chapter 11) of combinations of skill, life force, and genealogical and cosmological control offer rather compelling comparisons; only concepts of toughness and military force seem absent, though depending on how we interpret the walling of sites like Zambujal, Leceia or Los Millares, those could have been present elsewhere in southern Iberia. But if these are the would-be lords or chiefs of Valencina, their time in the sun was, according to the chronological models now available, strictly limited. Whatever label is best used at Valencina, the process of social differentiation is once again of limited duration. In this case, it is as if all the effort involved in mobilising labour, establishing and maintaining long-range connections, and affording craft skills and elaborate funerals, may have been too great to sustain for more than a few generations at most. If the emergent models are correct in placing the even larger, stone-vaulted tholoi such as La Pastora (Fig. 5.19) and Matarrubilla, with their divergent orientations, even later in the Valencina sequence, there may have been a lull in mortuary activity, and then a last gasp of extravagant mortuary activity may be as much to do with the encroachment of new, Beaker-related practices in Iberia as another phase of social differentiation (García Sanjuán *et al.* 2018).



Fig. 5.19: La Pastora. Left: part of the long passage; right: the stone-vaulted roof of the chamber. Photos: Leonardo García Sanjuán.

There is still much more to find out about this context, as with all the other case studies surveyed in this chapter. At some point in the future, it should be possible to put together detailed, precise chronologies for a whole series of sites across southern Iberia, across the varied contexts of other sites such as Perdigões, Leceia, Zambujal, Vila Nova de São Pedro, Camino de Las Yeseras, Marroquies Bajos and Los Millares (among many others, see Díaz-del-Río 2011; R. Chapman 2008; Kunst 2010; Cámara Serrano *et al.* 2012; García Sanjuán and Murillo-Barroso 2013; Valera 2016; Valera *et al.* 2014; Balsera Nieto *et al.* 2015; and references). For example, there may be walled enclosures in operation in the early centuries of the third millennium cal BC at Leceia and Zambujal in central Portugal (Schuhmacher *et al.* 2009; Kunst 2010) and at Los Millares in south-east Spain (among others, Molina *et al.* 2004; critically surveyed in R. Chapman 2008, 205), but whether they are precisely contemporary or lasted for equal periods of time, whether they were symbolic, functional or multi-purpose, and whether they are socially identical, are all questions which remain to be more reliably settled. I think the context as a whole was very varied: perhaps a repeating series of short-lived bids for power and position. Better understanding of the wider situation to which Valencina belonged will help in its own characterisation.

Provisional propositions

There is now a much bigger range of choices in the literature, of analogies and models for social formations and relations, which we may potentially want to apply to Neolithic European situations and trajectories of change. But it is not just a question of being more subtle or discriminating in the use of labels, though that would help, but of shifting our interpretive habits. Instead of reaching for the pre-formed generalisation or model, we need to work harder at particularising interpretation. This was the complaint of Tim Pauketat (2007) about the chieftdom model in Mississippian archaeology, and I believe that the need to shift from generalisation to individualising history applies also to the European Neolithic and Copper Age.

From a comparative perspective, perhaps much of European Neolithic sociality could be classed as ‘transegalitarian’, in the terms set out earlier in this chapter. That helps to identify the broad character of the long sequence of the European Neolithic, but it does not establish its more detailed history. There may be regions and phases where either stability or gradual change was the norm, and there is good reason to think that there was plenty of resistance to aggrandisers and would-be usurpers of communal values, but what I have called a gradualist approach need not apply everywhere. Sequences which can be smoothed out in the long term may often have been far more dynamic and fluid in the short term than we have been accustomed to envisage, and short-lived moves towards differentiation may have been more common than often supposed. Likewise, long-term continuity should not be thought of as a given, and reversals and collapses should be expected. The notion of cycling may be helpful, though the nature of cycles or trajectories need not be regular or repeating.

A useful rule of thumb may be that there is a tendency for moves towards social differentiation in its many possible guises to be short-lived, while correspondingly, long-lived and more or less stable situations may often reflect either a lack of pronounced imbalance in social relations or some kind of balance between the values of community and the efforts of would-be aggrandisers. Efforts to shift the nature of social relations and access to power are likely to have been unstable, given a strong set of pre-existing communal values which would have to be given up or otherwise circumvented. The resulting tensions between maintenance, subversion and resistance may in large part define the nature of the history of Neolithic social relations.

So this chapter has sought to show that probably no major

differences were firmly established for long. Despite their differing details, the sequences of Varna, Alsónyék, southern Britain in the earlier part of the fourth millennium cal BC, and Valencina de la Concepción could all be read in this way; the sequence in Orkney, discussed in the previous chapter, is another candidate (Bayliss *et al.* 2017). Some differentiation could flare up quickly, as it were out of nowhere, as may have been the case with Varna, but the potential for it could also have been embedded in many other situations and could have eroded established systems by their end, like the Vinča or LBK cultures, or disrupted fragile aggregations like Alsónyék or mainland Orkney. The situation in northern France in the later fourth millennium cal BC may speak to continuing traditions of communality, but in a context of increasing concerns for land and territory.

In the next chapter I will consider the impact that these different perspectives may have on our view of Neolithic history as a whole. Are there clear trends through time? Perhaps this was a much more dynamic, punctuated and in a sense ragged sequence throughout than we have normally envisaged.

Notes

- 1 One of the examiners of my D.Phil., in 1976. After an hour and a half of grilling me, he turned to the other examiner, Pieter Modderman, and suggested a pre-lunch gin and tonic. I passed.
- 2 I use Flannery and Marcus (2012) as a convenient source to illustrate the diversity of social relations with which we might have to contend in the study of Neolithic Europe. I have given further, primary references in some instances; many more are provided by Flannery and Marcus.
- 3 Using authors whose work I admire, so that this survey should be seen as a springboard to doing things differently in the future, rather than as negative critique. For insightful comments on Corded Ware and Bell Beaker sociality in central Europe, but still using the language of hierarchy and complex society, see Heyd (2007; 2016).
- 4 And there is a hive of current research activity based on Kiel University. Among many other references, see Müller and Hinz (in press).
- 5 For a whole series of reasons, this is unconvincing (García Sanjuán *et al.* 2018).
- 6 And see similar critique in Van Dyke (2015).
- 7 But the varied fills and frequent recuttings of the Okolište ditches are to be noted (R. Hofmann 2013, 474–5).
- 8 And marked reduction in the size of the settlement had already taken place at Okolište c. 4850 cal BC (R. Hofmann 2013).
- 9 A further ToTL paper is in preparation on the dating of these and other Lengyel sites: Regenye *et al.* in prep.
- 10 A further ToTL paper on the LBK in the central Polish lowlands is in preparation.
- 11 Wiederstedt also has a collective deposit, of mainly young people, but without definitive evidence of killings.

Chapter 6

An old Europe in a new light

Forks in the road: choices of history

‘When you come to a fork in the road, take it’, pronounced the late, legendary American baseball player Yogi Berra.¹ If the trails ahead of us are often confusing, the path which I have advocated we should now go down has a large sign beside it labelled ‘history’. I have tried to show in the previous chapters how increasing chronological precision enables much more detailed and fuller narratives than previously thought possible for ‘prehistory’, with a correspondingly much wider choice of scales; I have argued, ambitiously, that as a result we can write history for the Neolithic period in Europe, despite it being remote from us by several thousand years. Working with different scales, I want to wrestle in this chapter with the question of the kind of history or histories that we can construct.

Two brief examples from the work of leading historians (see [Chapter 2](#)) serve to provide a useful yardstick against which to measure the claim for the validity of historically meaningful narratives within Neolithic and other archaeology. *The pursuit of power: Europe 1815–1914* by Richard J. Evans (2016) offers wide synthesis and global perspectives, if not grand narrative. The structuring theme is that of power: imperial, military, economic, social, religious and environmental, exercised formally and informally, by violence or persuasion, and by consensus or majority; this is set against its foil, that of emancipation (R. Evans 2016, xx–xxi). Those central developments serve to organise lengthy and detailed (though at times rather compressed) narratives about political, economic and social changes through the century covered. These are interspersed with more thematic treatments. Evans includes, like Jürgen Osterhammel (2014) before him, all manner of topics, such as trade unions in Dorset, bear hunting in Hungary, and steam trains everywhere, using particular topics to illumine, on the one hand, the pace of modernisation (including photography, lifts, the trans-oceanic telegraph, and department stores) and on the other, the striking differences between 19th-century attitudes and those of today (such as

the widespread, socially enforced convention of wearing hats in public: R. Evans 2016, 503) and the real strangeness of a time not so very distant from our own. The volume opens with details of the sufferings of one man, Jakob Walter, whose memoirs recount his time in the retreat of Napoleon's Grand Army from Russia; other chapter openings also use tales of other particular individuals to bring a human face to the big processes under discussion.

In many ways, *The ordeal of Elizabeth Marsh*² by Linda Colley (2007) operates from a converse perspective, since it recounts what is known of the life, travels and adventures of one woman in the mid-18th century, in England and the Mediterranean as part of a family closely linked to the navy, in brief captivity in Morocco, and during a long final sojourn in India, where she and her husband had relocated following his bankruptcy in London. Where the two books converge to an extent lies in Colley's interest in the global context, seen through how the lives of one woman and her close family intersected with the many changes and rapidly intensifying connections being created around the world in the 18th century. Colley (2007, xxiii) lists, for example, 'enhanced maritime reach, transoceanic and transcontinental commerce, a more deliberate mobilization of knowledge and written information in the service of the state, the quickening tempo of imperial aggression and colonization, emigration, war, slavery and the slave trade'.

I also asked some of my historian colleagues in Cardiff University for personal selections of two or three books that, for them, captured all that is best about the writing of history. Neither of the two books cited above appeared in their responses. E.P. Thompson's (1963) classic, *The making of the English working class*, emerged as one favourite, and a very wide set of others were offered. These ranged from the close focus of works such as *King John* (Warren 1961) or *Bukharin and the Bolshevik revolution: a political biography, 1888–1938* (Cohen 1971), to more discursive treatments such as *Ireland and empire: colonial legacies in Irish history and culture* (Howe 2000) or *The most solitary of afflictions: madness and society in Britain, 1700–1900* (Scully 1993), to other forms of grand narrative, such as *After Tamerlane: the global history of empire since 1405* (Darwin 2007) or *London: a social history* (Porter 1994).

No doubt any other group of historians would come up with many further choices, but even from the small sample cited, the range of history writing is clear. Such diversity seems to me to make more than enough space for the claim to be able to write some kind of history in and for what has previously been labelled as 'prehistory'. I have

already identified at least three strands in this argument: a mutual interest in creating narrative, and the choices which that inevitably raises; the shared challenge of incomplete sources, such that the presence or absence of written records should not necessarily be the deciding word; and what should be a joint concern for particularising rather than generalising accounts of the past. But it seems unlikely that our archaeological narratives will be identical to those of the historians noted above (and see again [Chapter 2](#)), and we need to be confident in the power and reach of our own kind of narratives. What kind of history overall for the European Neolithic that can be, and how, forms the substance of this chapter. Working at this, and matters of timing and tempo, will inevitably bring us back to the question of different scales, and to thinking about how we can combine them.

A frame of major turning points

Beginnings

One way to start is to try to identify some of the major turning or tipping points (cf. Robb and Pauketat 2013, fig. 1.1; see again [Fig. 2.3](#)) which may have structured the overall shape of the Neolithic trajectory. Here we need to give full attention to the plethora of recent genetic studies, already mentioned in previous chapters. These now include analyses of the whole human genome, rather than just individual markers, predominantly mitochondrial ones in the female line (out of a much longer list, see Gamba *et al.* 2014; Lazaridis *et al.* 2014; Haak *et al.* 2015; Allentoft *et al.* 2015; Mathieson *et al.* 2015; 2018; Olalde *et al.* 2018; Lipson *et al.* 2017). Between them such aDNA studies have extended coverage across most – though not yet all – areas of Europe, including now the south-east (Mathieson *et al.* 2018; González-Fortes *et al.* 2017), and even some of the Near East (Fernández *et al.* 2014; Omrak *et al.* 2016). Between them, and with the many recent technical advances in aDNA analysis including the enhanced ability to combat contamination (Orlando *et al.* 2015),³ these sorts of study are helping to transform our understanding of the makeup of Neolithic populations ([Figs 6.1–6.2](#)). Papers are being published thick and fast, and no doubt further ones will be out during the production of this book. In all the excitement, it is worth noting gaps in current coverage, including in parts of western Europe, and despite the enormous increase in the amount of information that can be processed, the level of sampling can often still be low for some areas. As just one example, a single individual from late fourth-millennium cal BC Ireland (Cassidy *et al.* 2016; but see also Olalde *et al.* 2018) figures prominently in wider inferences for that region.

While there is thus certainly room for caution in the current state of

affairs, results after results now emphasise the separateness of the genetic makeup of hunter-gatherers and early European farmers, on the one hand, and the clear links between early European farmers and populations in the Near East, on the other, even though incomers into south-east Europe may not all have come at once (Mathieson *et al.* 2018; Lipson *et al.* 2017; see also Kulunc *et al.* 2016). One study has even put the figure of only 1.8 percent of LBK mitochondrial haplotypes matching hunter-gatherer lineages in a fairly wide analysis across central Europe (Brandt *et al.* 2013; 2015, 79).⁴ So much for the view that indigenous Mesolithic people could have been heavily and directly involved in the initial adoption of the Neolithic lifestyle (e.g. Whittle 1996), even though variation in the extent of their possible contribution does seem apparent as studies are extended (e.g. Mathieson *et al.* 2018; Gonzalez-Fortes *et al.* 2017). Instead, opportunities open up for thinking about how colonisers could have used the new conditions they faced, for example in central Europe (D. Hofmann 2015).

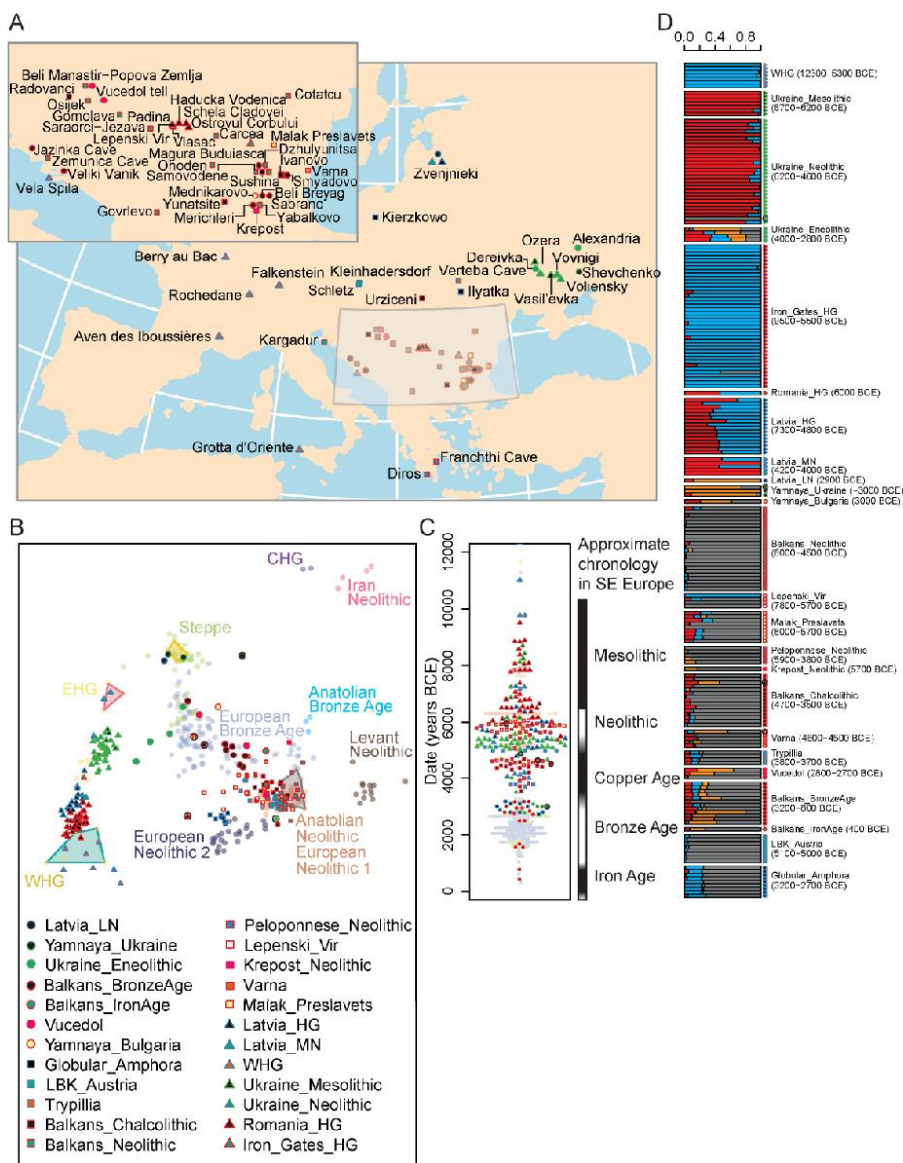


Fig. 6.1: aDNA analysis in action: a new study of human samples from eastern Europe (A), through time (C). These are compared to a large sample of modern West Eurasians (B), and differentiated (D), modelling each ancient individual (one per row), as a mixture of populations represented by clusters containing Anatolian Neolithic (grey), Yamnaya (orange), Eastern hunter-gatherers (EHG; red) and Western hunter-gatherers (WHG; blue). After Mathieson et al. (2018), by kind permission of Iain Mathieson.

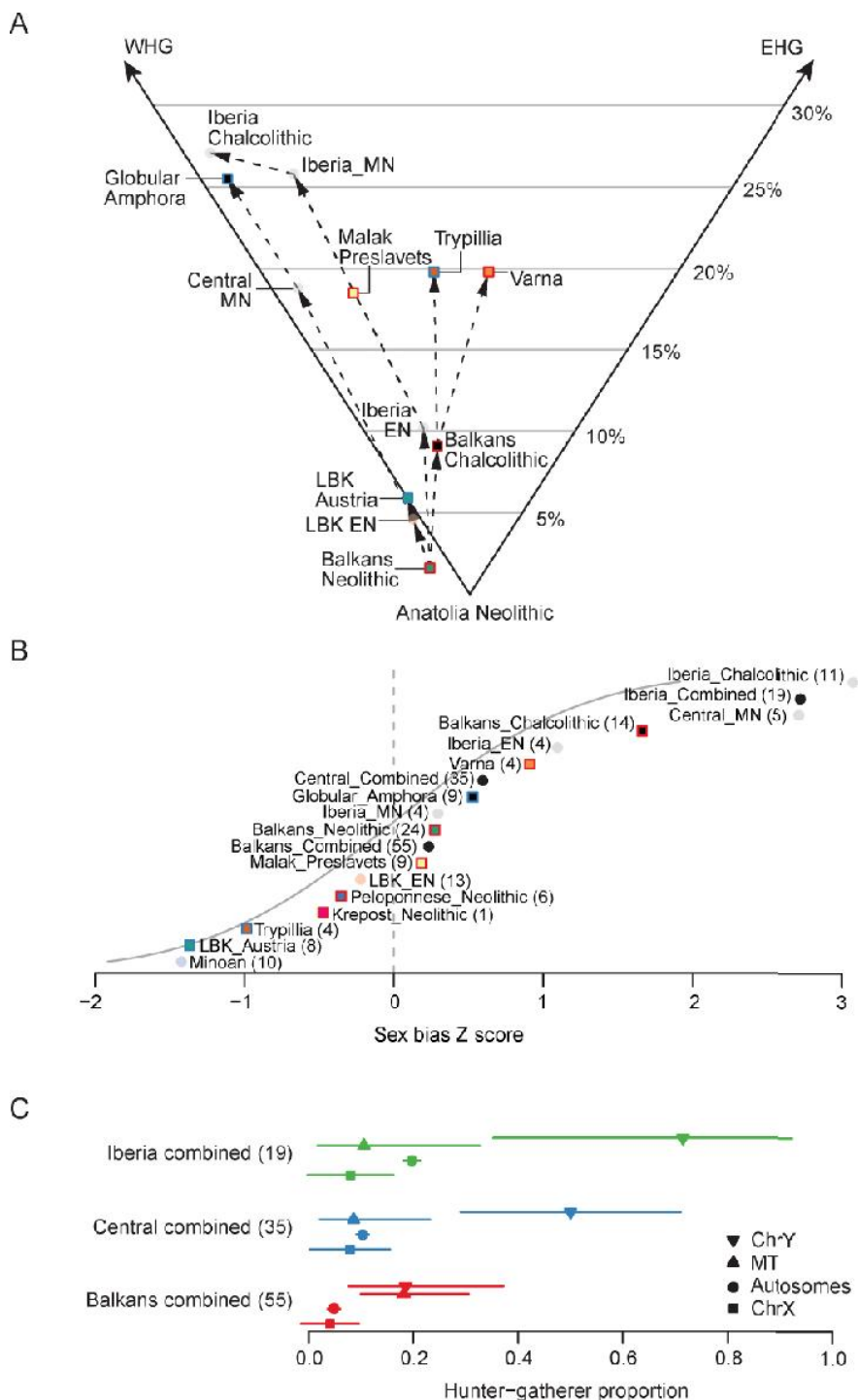


Fig. 6.2: *aDNA analysis in action: further interpretation of structure and population change in European populations with farmer ancestry, with (A) modelling of each population as a mixture of Anatolian Neolithic, WHG and EHG (dashed lines show temporal relationships between populations from the same geographic areas with similar ancestries), (B) statistical differences in hunter-gatherer*

ancestry on the autosomes compared to the X chromosome when populations are modelled as a mixture of Anatolian Neolithic and WHG (a positive score indicates that there is more hunter-gatherer ancestry on the autosomes and therefore the hunter-gatherer ancestry is male-biased), and (C) hunter-gatherer ancestry proportions on the autosomes, X chromosome, mitochondria (i.e. mt haplogroup U), and Y chromosome (i.e. Y chromosome haplogroups I2, R1 and C2). After Mathieson et al. (2018), by kind permission of Iain Mathieson.

At this stage in genetic research, however, the picture does not appear to be the same everywhere. One review, overlapping with the first genome-wide studies, has identified different processes at work in other parts of Europe: a bigger hunter-gatherer legacy in Iberia, perhaps consistent with small-scale, coast-hugging, colonisation by incomers, for example, and perhaps small-scale colonisation from the outside mixed with slow interaction with the indigenous population in southern Scandinavia (Brandt *et al.* 2015, 81–2; see also Szécsényi-Nagy *et al.* 2017). As for Britain and Ireland, analysis and comparison of the genome of the late fourth-millennium woman from Ballynahatty have given an estimate of predominantly early farmer ancestry, but with roughly 40 percent hunter-gatherer genetic makeup (Cassidy *et al.* 2016, 369). If valid for the population as a whole, that would neatly match the kind of mix of incomers and native people which we envisaged from our chronological studies (Whittle *et al.* 2011). The inference of close similarities between British and southern French and Iberian populations (Olalde *et al.* 2018, 193), however, does not fit our own suggested model so well; nor does it fit any other current model particularly well, though it could be more easily accommodated into the western strands of Alison Sheridan's models (2010), if an intermediate link between Iberia and Brittany were to be added.

Although there are therefore many details still to sort and gaps to fill in (Brandt *et al.* 2015), these and other results help importantly to reset the narrative for the beginnings of the Neolithic across Europe, which becomes again a dominant story of the effects of incoming communities. Perhaps such results will encourage European researchers to look further at the character of communities in Anatolia and other likely source areas in the Near East, where by the later seventh millennium cal BC there appears to have been a process of differentiation into smaller households, less centralised control of space and perhaps shorter house durations (Marciniak *et al.* 2015; Furholt 2016). If this shift was carried over into the colonisation process, it must have helped to set the emerging social dynamic in Europe. It is not my intention to follow this process in detail here (see the comprehensive survey in Robb 2013); that would be for another book. But I do want to draw attention to the timescales of transition and transformation, because I am not confident that these have been

rigorously established everywhere.

I have already referred to two examples from our own projects where formal modelling has radically challenged previously accepted views of the timing and tempo of transition. In Britain and Ireland, instead of a single transition around 4000 cal BC, a lengthier process lasting some three centuries and beginning in the 41st century cal BC was proposed (Whittle *et al.* 2011). In central Europe, instead of a blanket date around 5500 cal BC for the emergence and spread of the LBK as a whole, we have proposed that point as marking the initial formation of the LBK, followed by its rapid and wide spread in the 54th century cal BC (Jakucs *et al.* 2016). It remains to be seen what variations in tempo can be established elsewhere. With more excavations in western Anatolia and northern Greece, more radiocarbon dating and some formal modelling (Horejs *et al.* 2015; Reingruber 2015b; Douka *et al.* 2017), a picture is perhaps developing of a slow and regionally varied process of change in the earlier to mid-seventh millennium cal BC, speculatively consistent with smallscale, perhaps even sporadic, colonisation, by multiple routes (see again Mathieson *et al.* 2018), including over significant distances by sea (Fig. 6.3). From earlier dating projects (though without formal modelling), we know of Starčevo communities in the northern Balkans by the late seventh millennium cal BC (e.g. Whittle *et al.* 2002), which fits with the more precise and modelled chronology for changes in the Danube Gorges among a local population reacting strongly to the world altering around it (Borić 2016). What we still lack is a detailed sense of the pace of spread in the southern Balkans in the intervening centuries; my guess is that the tempo was slower rather than quicker, until the triggering of rapid change in the 54th century cal BC in central Europe and beyond.

It will be interesting to see if or how varying tempo is correlated in some way with genetic diversity. In the west Mediterranean, where aDNA results suggest a shared ancestry between LBK and Impressed Ware/Cardial populations (e.g. Gamba *et al.* 2012; cf. Olalde *et al.* 2018: for differences), a combination of more targeted radiocarbon dating (using short-life samples) and formal modelling has lowered the date estimates for the spread of what may be again small-scale colonising groups to the earlier sixth millennium cal BC, but intensified the speed of this process (Binder *et al.* 2017a; 2017b). Further north, there also appears to be a mixture of slow and fast changes. The LBK advance went only so far, and a much slower process of Neolithisation took place in the western part of the Paris basin and Brittany in the earlier fifth millennium cal BC, in the Alpine

foreland from c. 4300 cal BC, and in northernmost Germany and southern Scandinavia from the very late fifth millennium cal BC, matching beginnings in southern Britain. But there remain issues of precise timings in all these sequences.

These questions of timing and tempo matter. They must contribute to our understanding of the character and social composition of colonising groups. One general model has made the distinction between long-distance migration and short-distance migration, the latter like ‘wave of advance’ movement (Anthony 1990, 899). Long-distance migration often seems to involve prior scouting and subsequent targeted movement, not necessarily by whole communities (Anthony 1990); ‘cultures don’t migrate; people do’ (Anthony 1997, 27). Migration streams often derive from a single, restricted point of origin, and such narrow selection combined with changes in the new territory in which movers ended up can lead to radical stylistic change in material culture (Anthony 1990, 903). With a shifting limit to the distribution of the Neolithic way of life – I hesitate to use the term ‘frontier’ – but in a world of people on the move, as supported now by more reliable aDNA results and genome-wide analyses, we need to think again much more precisely about the speed of movement, and its social implications. The story was not necessarily one only of ‘slow, patchy but steady’ expansion across Europe (Robb 2013, 657). That might apply best to a gradual initial spread, perhaps much of it akin to a ‘wave of advance’, through south-east Europe and into the central Mediterranean, and perhaps largely the work of small groups or communities, more or less undifferentiated in terms of social relations.

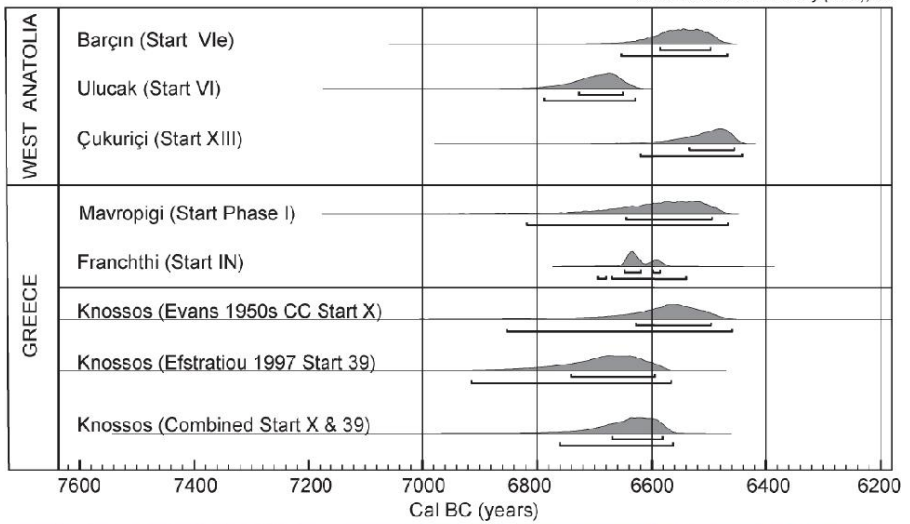


Fig. 6.3: First appearance of the Neolithic around the Aegean. After Douka et al. (2017).

Something different seems to have been at work in the very rapid expansion of the LBK into central and western Europe (see again Fig. 5.8). Should we class this as long-distance migration, or an accelerated wave of advance? Newcomers seem to have known where they were going, and although genetic results appear to be largely cutting out indigenous people, perhaps there is a more subtle relationship at work in terms of knowledge of the landscape and previously established networks of communication, seen for example in the pre-LBK distribution of radiolarite from its source in north-west Hungary up into central Europe (Mateiciucová 2008). The suggestion of leading lineages or clans behind initial LBK colonisation (Friedrich 2005) fits

well with the much increased tempo of this process. Whether that works so well for the suggested targeted coastal colonisation of the west Mediterranean remains to be seen; potential expressions of social difference in the form of the construction of megalithic tombs appear to follow quite some time after initial colonisation and establishment of a Neolithic lifestyle (Balsera Nieto *et al.* 2015). The same appears to apply in Britain and Ireland (Whittle *et al.* 2011).

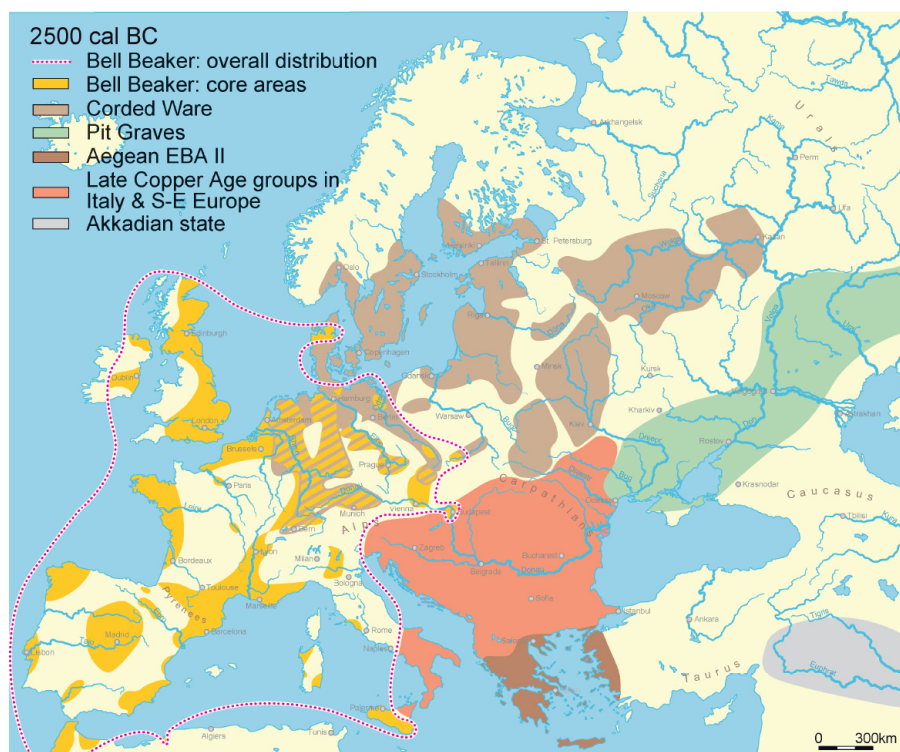


Fig. 6.4: Map of major cultural groupings across Europe c. 2500 cal BC. After E. Kaiser (2017), based on Müller (2009), and by kind permission of Johannes Müller.

Twice upon a time: endings

In the flow of history, any definition of beginnings and endings risks being relative or arbitrary, depending on the nature of the narrative being created, and this applies perhaps particularly to the notion of endings. Some scholars have chosen to emphasise changes in the so-called Copper Age succeeding the Neolithic (Lichardus and Echt 1991), but with a sliding chronological scale on which copper metallurgy and associated practices are introduced, that is hard to defend as a neatly definable entity (Schier 2014). What I want to do here is simply to underline the series of changes across many parts of Europe in the third millennium cal BC which can be taken for the

purposes of my narrative as a significant transformation of the existing Neolithic world (or worlds). With the newly available genetic evidence, this appears again to involve the movement of people (Haak *et al.* 2015; Allentoft *et al.* 2015). This affects a very broad swathe of eastern, central and part of western Europe, in the form of the Yamnaya and Corded Ware cultures (Anthony 2007; Kristiansen *et al.* 2017; Heyd 2017) (Fig. 6.4). There are further and subsequent third-millennium changes in the guise of the Beaker complex, which may also involve, in some parts of its distribution, significant movements of people (Olalde *et al.* 2018).

Authoritative recent surveys of the Corded Ware complex (Furholt 2014; Heyd 2016; 2017) have stressed both regional diversity across its distribution and the signs of continuity with pre-existing practices, for example in the Globular Amphora culture, in terms of both elements of material culture and mortuary practice. On a wider geographical scale, there are other signs in the later fourth and early third millennium cal BC, for example in the world of the Ice Man around the fringes of the Alps, of particular individuals and identities being picked out by distinctive stelae and other practices (Robb 2007; Harris *et al.* 2013). The distribution of Corded Ware groups is also patchy across central, northern and western Europe (where it effectively stops at the Rhine) (Furholt 2014, fig. 6). The details of mortuary practice, one of the defining characteristics of the complex, vary quite markedly from region to region (Furholt 2014, 75–6), as well as the defining sets of beakers, amphorae and stone axes (Furholt 2014, 76–7). The previously supposed early horizon of uniform material culture (the so-called *Einheitshorizont*) probably now does not sit at the start of the Corded Ware sequence but may plausibly be a subsequent development (Furholt 2014, 72–5). A major conclusion of one review, therefore, was that ‘it makes no sense to try to find a single explanation for the spread of the elements referred to as [the] Corded Ware phenomenon’ (Furholt 2014, 81). It was further argued that no anthropological model would support any claim for a totality, ‘except crude concepts about ethnicity and mass migration of uniform social units’ (Furholt 2014, 81). An alternative notion of ‘a set of symbols and practices that were selectively incorporated into, and transformed by, local societies’ is preferred (Furholt 2014, 67), within a context of widening and intensifying networks of contact and exchange (Furholt 2014, 82); very long-range interconnectivity is also stressed in another account (Heyd 2017; see also Jeunesse 2017b).

There is no denying the diversity evident across the Corded Ware distribution. In our own ToTL review of aspects of settlement in the

Alpine foreland, we stressed on the one hand the evidence for considerable local continuity alongside the appearance of Corded Ware pottery, and on the other the clear differences in the same horizon between northern and western Switzerland (Ebersbach *et al.* 2017). In the Alpine foreland, Corded Ware pottery appears on settlements in the 28th century BC, supported by dendrochronology (summarised in Ebersbach *et al.* 2017; see also Stöckli 2016). We should note, however, that Corded Ware radiocarbon chronology as a whole leaves much to be desired (Włodarczak 2009) and a major international programme of further dating and formal modelling is surely called for. While the quality of previous samples has been noted (Włodarczak 2009), informal estimates of varying start dates for the different regional sequences take no account of scatter (Steier and Rom 2000; Bayliss *et al.* 2007).

The new genetic results for Corded Ware and Beaker populations, in contrast, including wide sampling and genome-wide analyses (Haak *et al.* 2015; Allentoft *et al.* 2015; Olalde *et al.* 2018), have been taken to assert ‘massive’ migration from the steppes, given the introduction of new genetic signatures and even the fading away of the earlier ‘European farmer’ signature (Cassidy *et al.* 2016; Olalde *et al.* 2018). Once again, caution at this stage is appropriate, while potentially simplistic equations between genetic makeup and lived identity are debated (Vander Linden 2016). But some archaeologists are already not shy of starting to re-think forms of Yamnaya (Figs 6.5 and 6.6) and Corded Ware mobility and migration, for example by more mobile and more warlike incoming pastoralists, some of them in the shape of organised war-bands of young males, with an economic and social dominance expressed in wife-taking from farming communities, and with the use of waggons as mobile homes or of light dwellings portable on pack animals (Kristiansen *et al.* 2017, 337, 340). Significant population movement into Britain is also argued in the Beaker horizon (Olalde *et al.* 2018), though that seems at odds with the totality of the available isotopic evidence for lifetime mobility (Parker Pearson *et al.* 2016).

There are interesting parallels in the development of models for both the LBK and Corded Ware cultures. In each case, arguments that regional diversity of practice supports the existence of essentially local or autonomous populations (e.g. Bickle and Whittle 2013; Furholt 2014; Heyd 2017) have been challenged and subverted by new genetic results for newcomers on a considerable scale (D. Hofmann 2015; Kristiansen *et al.* 2017). As one paper has neatly put it, we should be sparing in our use of Occam’s razor (Haber *et al.* 2016).

Regional variation in mortuary practice need not be incompatible with a fractured process of migration and infiltration by new population, nor need pre-existing processes and linkages preclude subsequent arrivals. It becomes all the more pressing from that perspective to establish now a much more robust and precise chronology for the late fourth and early third millennia cal BC, in order again to track the trajectory and tempo of change. This applies not only within the Corded Ware distribution, but in the areas around it. In an increasingly connected late Neolithic world, developments in one region may often have produced an effect on or reaction in neighbouring ones. There is no significant Corded Ware presence in northern France, for example, but I believe that it is not just coincidence that the earlier third millennium cal BC, the phase of the *Néolithique final* of the Paris basin (Salanova *et al.* 2011), sees the appearance of both very large houses (Tinévez 2004) and a series of much more individualised burials within the existing, previously collective tombs or *allées sépulcrales*, for example at Bury, Oise (Salanova and Heyd 2009; Salanova *et al.* 2017). The dramatically large-scale feasting event (or events) in the last gasp of the history of the Ness of Brodgar settlement complex in Orkney could be related to the disruption of existing networks caused by the emergence of the Beaker complex (Card *et al.* 2017; Clarke *et al.* 2017; Sheridan *et al.* in prep.). How far such ripples went is a matter for debate. It has been claimed, as noted in the previous chapter, that the disposition and orientation, and the character of accompanying grave goods, in the 10.049 tholos burial in the PP4-Montelirio sector within the Valencina complex reflect Corded Ware mortuary practice (Heyd 2017, 354; cf. Harrison and Heyd 2007), though this may be a step too far, and anyway begs questions of precise chronology.



Fig. 6.5: Adult female in a Yamnaya grave originally under a small mound at Bojt-Tökös Varga-tag 3, near Debrecen, Hungary (Dani et al. 2017), by kind permission of Volker Heyd.

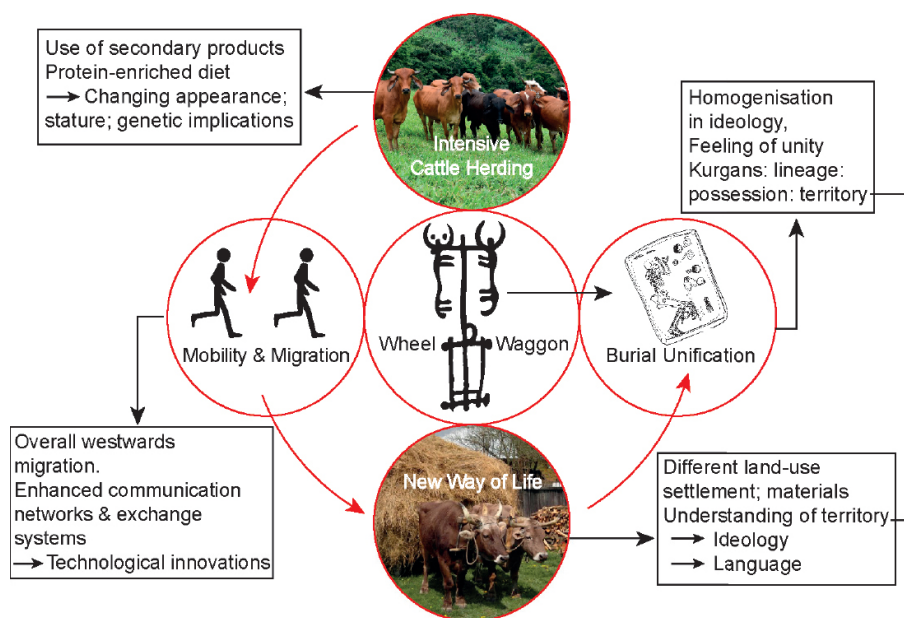


Fig. 6.6: *The world of difference: Yamnaya innovations*. After Frînculeasa et al. (2015), by kind permission of Volker Heyd.

The big stories in the middle?

If both the beginning and end of the Neolithic narrative defined here are now looking significantly different to their characterisation by many scholars over previous decades of research, what other major turning points can be identified in between these points, and has our understanding of their nature also changed with recent research? Genetic study in central Europe and the Carpathian basin has suggested two phases of integration of hunter-gatherer populations back into farming communities (Brandt *et al.* 2013; 2015: ‘events B1 and B2’; Szécsényi-Nagy *et al.* 2015; Mathieson *et al.* 2018). This is intriguing but puzzling, given the now apparent non-involvement (or substantially reduced engagement) of indigenous people in initial transformations, but the possibility of long-lasting enclaves of hunter-gatherers has been supported by the genetic study of the Blätterhöhle cave in north-west Germany (Bollongino *et al.* 2013). Nonetheless, this appears as a gradual process played out over time, rather than a single tipping point. What I have in mind are at least two horizons of congruent changes, so widespread that they are often considered by different research communities who do not always speak to one another.

The first of these is in the later sixth millennium cal BC. Now by this date, as discussed above in this chapter, we could envisage a rolling edge to the spread of new people, things and practices in south-east

Europe. The first newcomers had already passed into the central Mediterranean before this date, and not long before into the west Mediterranean. In the Carpathian basin, in western Hungary, we know of Starčevo population in the first half of the sixth millennium, as at the large site at Alsónyék in southern Transdanubia (Oross *et al.* 2016a) but there is not enough information yet to track the spread of Starčevo settlement in detail through space and time. In eastern Hungary, on the Great Hungarian plain, however, informal estimates of dates for Körös sites combined with newer discoveries up the Tisza valley do suggest a steadily rolling, northward spread of Neolithic settlement (Domboróczki 2010, fig. 11). There follows the formation of the LBK, apparently in western Hungary and eastern Austria around 5500 cal BC, as discussed above, and then the great diaspora of the LBK into central and western Europe, formally modelled as belonging to the middle decades of the 54th century cal BC (Jakucs *et al.* 2016). This also coincides, more or less exactly, with the formally estimated date for the emergence of the Vinča culture (Jakucs *et al.* 2016; Whittle *et al.* 2016). Previously, not only were the estimated timings of both developments different (as well as less certain), but they were generally treated as part of two separate narratives. On the one hand, here was a major development in the overall spread of the Neolithic way of life across temperate Europe, while on the other there was the start of consolidation, growth and settlement infill in a substantial part of south-east Europe already colonised and settled.

I think there is now a strong case for thinking about these two horizons together. As already discussed, the first prompt is that people in southernmost Hungary, as seen at the sites of Szederkény and Versend (Jakucs *et al.* 2016; 2018), were using a range of material forms: on the one hand, longhouse architecture in what would normally be recognised as LBK style, and a varied repertoire of ceramic styles, definable as early Vinča, LBK and earliest Sopot. Secondly, though it is not certain that the Vinča network emerged first in the northern part of its distribution, the evidence is certainly suggestive of that possibility (Whittle *et al.* 2016, 28, 40). The genetic evidence noted above now strongly supports incoming population as the principal constituents of the LBK, whether these derive from a Starčevo or a Körös culture base, or from both, and the clear implication (cf. Szécsényi-Nagy *et al.* 2015) is that genetically speaking it was more or less the same population that constituted the LBK and Vinča complexes. Now there has been a strong research tradition which believes in the formation of the Vinča culture by new population coming in from long range to the south, from even as far

away as Anatolia (see Whittle *et al.* 2016 and references). There are some signs from limited analysis of new genetic signatures in the eastern part of the Vinča distribution (Hervella *et al.* 2015), but not on a scale that really supports the traditional model of dramatically new Vinča population (and see Mathieson *et al.* 2018). If that is the case, the emphasis presumably has to shift to a phase of rapid population increase and cultural transformation, including initial fluidity in part of the distribution. Populations were growing, people were on the move, and new cultural identities were being formed very quickly. To be fair to the older research tradition, however, it is not clear at this stage if further population additions from the same source – that is, Anatolia – as at the start of the Neolithic could be detected genetically.⁵

The second potentially significant and widely documented turning point takes us to the mid-fifth millennium cal BC. As discussed in the last chapter, a mixture of informal date estimates and formal models suggests the 47th and 46th centuries cal BC as the time when many tells in the northern part of south-east Europe were abandoned (Borić 2015a). Formal models tie this down more precisely in the case of Uivar (Draşovean *et al.* 2017; and see further below), Okolište (R. Hofmann 2013) and Vinča-Belo Brdo (Tasić *et al.* 2015; 2016a; 2016b). The last Vinča occupation at the latter falls in the 4540s or 4530s cal BC (Tasić *et al.* 2015), and the end of the use of Vinča D pottery is estimated as belonging to the mid-45th century cal BC (Whittle *et al.* 2016, 32). Rapid aggregation and then slow decline have also been documented and formally modelled at Alsónyék in Transdanubia (Osztás *et al.* 2016b; Bánffy *et al.* 2016). The many possible reasons for this wide set of changes and general demise have been discussed above; what succeeds is a pattern of much more dispersed settlement (Borić 2015a; Parkinson 2002).

A different research community investigates the LBK and post-LBK tradition in central and western Europe, and yet another one (generally speaking) the emergence of the Neolithic around the fringes of Atlantic Europe and the appearance of monumentality. As discussed earlier, the timings of ‘Danubian’ endings in central and western Europe are not yet settled, nor the dates for the first monuments. In our ToTL study of Racot in the Polish lowlands, substantial longhouses were still in use as late as the last centuries of the fifth millennium cal BC (Czerniak *et al.* 2016) but elsewhere, for example in Lower Alsace, the last visible longhouses belong to the Grossgartach phase, dated to the late 48th to the earlier 47th centuries cal BC; by comparison with neighbouring areas they were probably still in use in the Rössen

phase, dated in Lower Alsace from the mid-47th to the earlier 45th centuries cal BC, though their presence cannot be directly documented in Lower Alsace (Denaire *et al.* 2017; Anthony Denaire, *pers. comm.*). A much wider study of the precise chronology of the demise of the longhouse is called for. Passy monuments in the Paris basin and the first constructions in coastal Brittany, as discussed in [Chapter 4](#), could also belong in the middle of the fifth millennium cal BC, though the lack of precision for the latter in particular is regrettable.

Undeniably, important questions remain about the timings of all these features, but it seems open for discussion on the basis of available indications that the endings of tells in the northern part of south-east Europe, the decline of major Lengyel aggregations, the beginning of the demise of longhouses and the appearance of the first monuments in northern and north-west France, and indeed of rondels in central Europe, could all fall in the same horizon between c. 4700 and 4500 cal BC. That is for rigorous further testing in the future. But if this is so, is this just a coincidence, or could such changes be linked in some way? Each research community tends to debate what seem like its own concerns. I have already touched on many of the main issues in [Chapters 4](#) and [5](#): the question of the rise of the autonomous household in the demise of tells; the social tensions likely within a major and rapidly formed aggregation such as Alsónyék, and the possible pressures of one kind or another which may have pushed people together in such numbers in the first place; and the beginnings of more explicit social differentiation in mortuary ritual and the construction of the first monuments. Curiously, the abandonment of longhouses has been less discussed, though the possible links between memory of longhouses and construction of some of the first monuments have often been debated. One suggestion for the sequence in the Paris basin is that Villeneuve-Saint Germain houses (perhaps not dated later than c. 4700 cal BC: Dubouloz 2003) were more focused on the present and on making an impact through size and impressive entrances than their LBK (*Rubané*) predecessors, while at the same time perhaps more inter-linked in terms of cross-cutting activities (Bickle 2013, 163, 169). Such VSG settlements were often quite small, and perhaps shorter-lived than some *Rubané* predecessors (Bickle 2013, 172). *é*

If there is a common thread running through these different situations, it is perhaps not in this instance factors such as population growth or climatic change (to which some researchers regularly turn), but shared concerns with central questions of community and social relations. On this view, this mid-fifth millennium cal BC horizon

would have been a time of widespread shifts in the nature of how people lived together, even though specific circumstances demonstrably varied from region to region. Whether there was any direct causal relation between regions is open to question. Why or how would the end of a tell on the Great Hungarian plain have any impact on the nature of settlement aggregation in southern Transdanubia, and why or how would shifts in numbers of people living in Lengyel houses influence the fate of post-LBK households far to the west? Perhaps that is to look for the wrong sort of linkage. This was a connected world, one of the best witnesses to which is the Alpine jadeitite in Varna graves of the same sort of horizon being discussed here, noted earlier. So it is not implausible to suppose that people were aware of what was going on elsewhere, even though local or regional circumstance must have been at the heart of what happened on the ground.

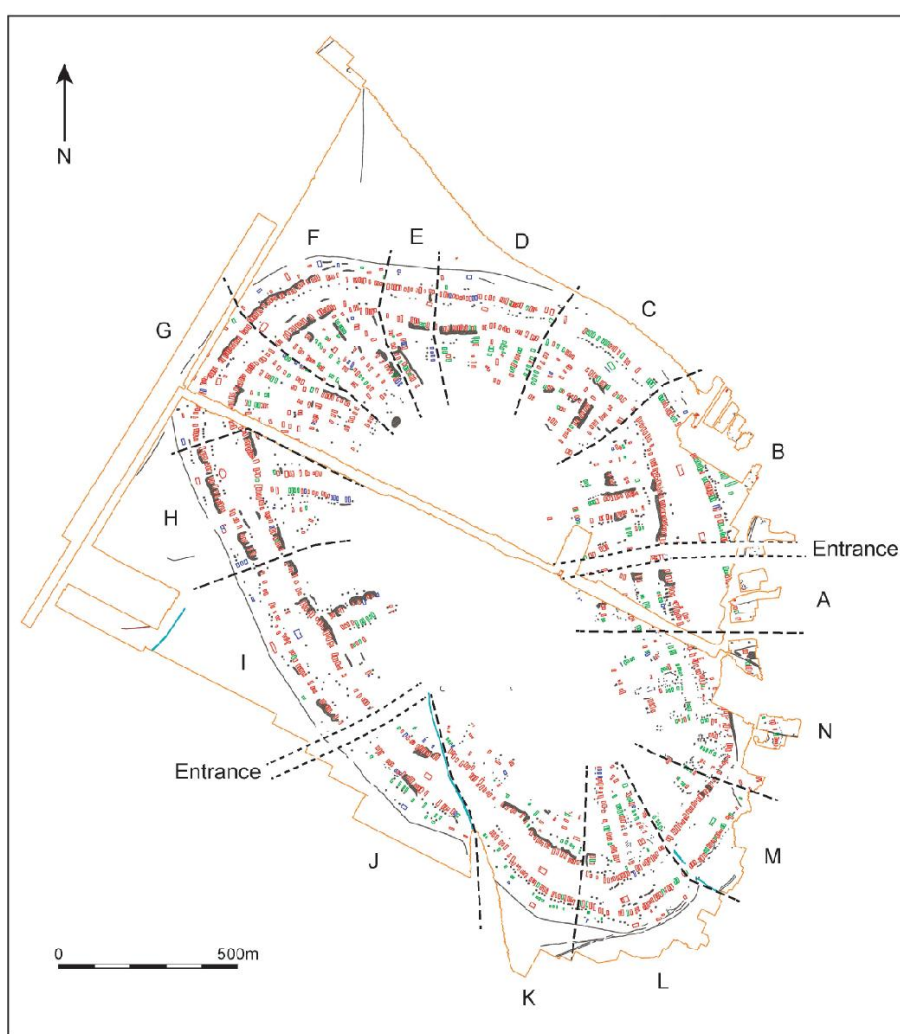


Fig. 6.7: Plan of the major early Trypillia settlement at Nebelivka, Ukraine. By kind permission of Bissierka Gaydarska and John Chapman.

So far, I have explored beginnings and ‘endings’, and two candidates for horizons of widespread changes, even though questions of linkage and causality are complex and unresolved. One might claim a kind of periodicity in these points through the millennia, with structural changes occurring every few hundred years, as already predicted by John Robb and Tim Pauketat (2013). On that basis, is there a similar horizon of widespread change between the mid-fifth and the early third millennium cal BC, somewhere in the fourth millennium cal BC? This is even harder to answer. Once again, different research communities tend to study different research questions: the end of tell settlements and their aftermath in the southern part of south-east

Europe (Parzinger 1993; D. Bailey 2000; Link 2006; Anthony 2007; Anthony and Chi 2010; Borić 2015a); major enclosed aggregations of the Trypillia culture out on the valley interfluvies of Ukraine (Menotti and Korvin-Piotrovskiy 2012; J. Chapman *et al.* 2014; Müller *et al.* 2016a); and a host of regional sequences in central and western Europe. In contrast to my suggestions above about earlier points of connected change, it is less easy to discern a single horizon of transformation.

Perhaps a case could be made for the early part of the fourth millennium cal BC as one such horizon, but probably this is forcing the evidence. Though this has yet to be widely tested by formal modelling, it looks as though many if not virtually all tells in the southern part of south-east Europe were abandoned by the end of the fifth millennium cal BC, with much more dispersed settlement in various subsequent phases such as those of the Cernavoda culture of the eastern Balkans or the Coțofeni culture in Romania (Anthony 2010, 48; and see many other references including Frînculeasa *et al.* (2015); Gogâltan 2013; Sava (2015)); the relatively few modelled estimates so far, including for tells such as Pietrele in the lower Danube, are consistent with this claim (Reingruber 2015a). A more robust absolute chronology is emerging for the Cucuteni-Trypillia sequence to the east, according to which after a long period of settlement going back to the late sixth millennium cal BC and shifts in the main foci of distribution, the distinctive enclosed ‘mega-sites’ appeared from the latest fifth into the fourth millennium cal BC on the interfluvium of the Southern Bug and Dniester⁶ rivers (Müller 2016). Of these, Nebelivka is so far the earliest, in the first two centuries of the fourth millennium cal BC (J. Chapman *et al.* 2016; Müller *et al.* 2016b, fig. 4; there is an active debate about the duration of this and other such aggregations) (Fig. 6.7).

Many possible explanations have been canvassed for the end of tells, including some kind of catastrophe or migrations from the east (Anthony 2010, 48). One view of the mega-sites has been as defensive sites in the face of migrations, both from the east in the form of the Yamnaya culture and from the west in the form of the Globular Amphora culture (Diachenko 2016, 275), though that presumably takes us much later into the fourth millennium, or even into the third. The chronology seems the wrong way round for some kind of domino-effect model involving movement from the steppes at the end of the fifth or start of the fourth millennium. Even larger mega-sites such as Maidanetske come later than Nebelivka, in the 38th century cal BC (Müller *et al.* 2016b, fig. 4), and so are comfortably later than the end

of tells in Bulgaria and Romania. That is not to exclude changes from the east in the course of the fourth millennium cal BC. But there are few signs in the latest genetic study of significant steppe ancestry coming into south-east Europe at this point, though there is a steppe signature in the Varna cemetery in the fifth millennium (Mathieson *et al.* 2018). I think next of the ‘traction complex’ (the plough and wheeled vehicles, which I discuss in more detail below), but its earliest manifestations, in the Baden culture of the Carpathian basin and developed TRB contexts further north, fall in the middle of the fourth millennium.

This is not the place to track every regional sequence going west. There are significant changes, as noted and discussed in earlier chapters, in western Europe and in Britain and Ireland in the late fifth millennium cal BC and into the early centuries of the fourth. These are probably connected, given for example the probable beginnings of the Neolithic in south-east England, which may in turn be related to an expansion of Michelsberg settlement on the adjacent continent (Whittle *et al.* 2011; but see again Olalde *et al.* 2018). This coincides with the expansion northwards of Neolithic people and practices, and the formation of the TRB culture. But though this is part of a very broad regional trend – and it is open to discussion whether settlement intensification in southern France for example, in the southern Chasséen culture is part of the same general development – it is hard in this instance to make even wider connections. The trajectory in the northern Carpathian basin had already been set by the earlier ending of tell occupations and the appearance of more dispersed settlement (in the Tiszapolgár and Bodrogkeresztúr cultures: for issues of potential chronological overlap, see Raczky and Siklósi 2013). The wider connections that are implied by the spread of the traction complex belong later: to the mid-fourth millennium.

Framing a history

These suggested major developments serve to sketch key parts of Neolithic history at a very large scale. It might be objected that in essence little has changed in our understanding of this kind of broad narrative: have we not long known that the Neolithic way of life arrived in Europe, and gradually led to many subsequent developments? On the contrary, I believe that the combination of wider genetic analysis and more precise chronologies allows us to outline a rather different kind of story to that often offered, in which slow change, continuity and indigenous or local agency had been dominant motifs. This was a world in which people could be on the move, newcomers being a central feature of the major spreads of the

Neolithic way of life from the earlier seventh to the late fifth millennia cal BC, and then again in eastern and central Europe and as far west as the Rhine in the early third millennium cal BC, with further movement again in parts of the Beaker complex in the middle and later part of the third millennium cal BC. In part, that confirms and refines older readings of the evidence, but not all previously suggested migrations are supported by the newer and ongoing genetic analyses, such as the ideas that the Vinča culture derived largely from new population to the south or the Iberian Copper Age from population from the eastern Mediterranean (but the suggestion of Jean Guilaine (2017), in a review of the Montelirio monograph, that very active contacts with that area can be seen in high-quality craft products at Valencina de la Concepción, should certainly be noted). Conversely, there is evidence for the varied, now rather muted, involvement of indigenous people at the start of the Neolithic, and then signs of a resurgence of hunter-gatherer genetic ancestry in the signatures of established populations of the fifth and fourth millennia, even though that is hard to track in the majority of the material evidence.

Such a narrative needs a timescale, and there is much work to be done to make this more robust and more precise. But enough has probably already been achieved to suggest the varying tempo of change. Probably slow and regionally varied beginnings may characterise the start of things in Greece and the southern Balkans in the earlier and mid-seventh millennium cal BC, and there was probably also gradual spread thereafter, both northwards through the Balkans and westwards into the central and then the western Mediterranean. This contrasts sharply with the explosion of LBK settlement in the 54th century cal BC, and again with the renewed expansions of the late fifth millennium in western Europe. Better timings also set the pace for the steady intensification of phenomena like the Vinča complex and the growth of tells, whose long histories come to an end in the eventful horizon of the 47th and 46th centuries cal BC in the northern part of the Balkans and the Carpathian basin, and again by the late fifth millennium cal BC in the southern Balkans. Slow developments could alternate with accelerated change and even rupture, and local concerns which played out at a steady pace contrast with much wider turning points and connections, as I have argued especially for the later sixth, mid-fifth and early third millennia, though in more regionally staggered form in the early and mid-fourth millennium cal BC. Even at this scale of narrative, slow change, continuity and local agency do not capture the whole character of what was going on in the Neolithic story.

Other times: the slow worlds of daily life, subsistence and people on the ground

What I have given above is an outline of a big-picture perspective, which serves, I believe, to give a new look to the history of the European Neolithic. But this is not all we need to consider. Back in [Chapter 2](#) I raised the question of different scales of human action and how to integrate them, so next I want to reflect on other dimensions of Neolithic existence – especially the playing out of life at daily and annual scales, the generally slow pace of subsistence change and the perhaps mainly steady pulse of population growth.

Daily lives

The perspective of daily life can seem to offer a very different kind of Neolithic to that given in the narrative above, because it may serve to emphasise continuity, endless repetition of the basics of life and a slow tempo to the unfolding of deep time. When I was reading widely in preparation for this book, I came across a striking description of the pattern and character of daily life among the Konyak Nagas of north-east India (Fürer-Haimendorf 1969, 35–9). Arguing now for a more historical and particularising approach, I have tried to avoid my previous attraction to and reliance on ethnographic analogy. However, this particular account of people's daily lives – rising early, preparing food, getting ready in leisurely fashion for work in their fields, walking some distance to those, labouring on and off, but not relentlessly, through the main part of the day, while not much happened back in their village until the various groups and work gangs returned in late afternoon – did strike a chord. This cannot stand for some kind of timeless agricultural existence; the Konyak Nagas practised slash-and-burn agriculture at the time of Fürer-Haimendorf's observations in the 1930s, their daily patterns were structured in part by age and gender, which might well be different in other settings, and there were seasonal variations (Fürer-Haimendorf 1969, 37–9). Nonetheless, the scenario seems very familiar. Rest and sleep, the preparation of food, tending to crops and animals, and working in smaller or larger social groupings ([Fig. 6.8](#)), interspersed with the diversions of 'numerous festivals and ceremonies which interrupted the routine of farm work', as Fürer-Haimendorf (1969, 35) put it, must have taken up the energies of most Neolithic people for the majority of their lives, playing out across the year to different rhythms (Mlekuž 2015).



Fig. 6.8: The communality of daily life in the modern Romanian villages of Măgura and Schitu. Photos: Douglass Bailey.

Making a living

That is a big generalisation, and the detail must have varied from place to place and from times to times, even if we do not quite know what kind of narratives to fashion from the welter of specialist reports on houses, settlement layouts and faunal and floral remains. One favourite kind of narrative for subsistence has involved discernible and directional change through the course of the Neolithic, still seen perhaps most clearly in the model of the ‘secondary products revolution’ (Sherratt 1981; 1987). As is well known, Andrew Sherratt proposed a set of changes which were introduced, ultimately from the Near East, well after the initial Neolithic, in the fourth millennium cal BC; a combination was first proposed of the traction complex, including the plough and the wheel, dairying and wool, to which he later added alcohol and drugs. Together these were seen as leading to both more specialisation and intensification of production. This range of innovations has been debated many times since (for example, Marciniak 2011; Greenfield 2005; 2010; 2014). Further research has served, in my view, both to dissipate the sense of a single or major horizon of innovation and development, and to bring other factors into consideration which were not discussed in the formulation of the original secondary products model.

Very briefly, we do not know when the plough was first introduced. Certainly the conditions for the preservation of ploughmarks markedly

improve with the appearance from the earlier part of the fourth millennium cal BC in western Europe, including northern Germany, southern Scandinavia, and Britain and Ireland, of monuments whose mounds can cover traces of cultivation in buried soils. It is not to be excluded that the plough was a tool available to earlier cultivators, of gardens and small plots (Bogaard 2004). Our understanding of dairying has been transformed by lipid analysis of pottery (Evershed *et al.* 2008). This shows the use of dairy products from a much earlier date than Sherratt originally envisaged, back into the Körös culture and the LBK (Craig *et al.* 2005; Salque *et al.* 2013), but their uptake may have been variable. Ruminant dairy fats are common in fourth-millennium pottery in Britain for example (Copley *et al.* 2005), but evidently are not omnipresent in the LBK (Matlova *et al.* 2017; see also Debono Spiteri *et al.* 2016). The latest genetic research indicates that the mutation required for lactose tolerance was not widespread in the population perhaps till the second millennium or later (Mathieson *et al.* 2015; Olalde *et al.* 2018), so that it may have been dairy products such as cheese⁷ which were consumed, rather than milk (Salque *et al.* 2013; see also Gillis *et al.* 2017); on the other hand, any ability safely to consume milk may have given particular individuals or groups various advantages. There appears to be no clear evidence for the substantial use of woollen textiles before the second millennium (cf. Marciniak 2011, 119; cf. Frei *et al.* 2017).⁸ Conversely, it seems very difficult to pin down the first use of alcohol or fermented drinks in any number of possible forms, and likewise drugs; cereal-based beer drinking by early farmers seems entirely possible. What does seem to stand up, as perhaps an eventful or event-like horizon, is the appearance of the wheel from the mid-fourth millennium cal BC onwards. This draws together the well-known Baden culture waggon models of the Carpathian basin (and Boleráz predecessors), other models of oxen pairs in developed TRB contexts, the linear marks interpreted as cart tracks within the Flintbek LA3 monument in northern Germany, formally modelled as belonging to the 35th century cal BC, and preserved wheels, yokes and travois in the Alpine foreland from the later centuries of the fourth millennium cal BC (Sherratt 1981; Ruoff and Jacomet 2002; Pétrequin *et al.* 2006; Anthony 2007; Bogucki 2011; Mischka 2011; Bogaard 2011, 273; Mauvilly 2012); the case has been made for a slow spread of the traction idea within the Alpine foreland itself (Pétrequin 2005, fig. 9) (Fig. 6.9). Evidence for traction animals elsewhere might, however, go earlier (Bogaard and Halstead 2015, 393). I have already noted the suggestion that wheeled vehicles could have been used by Corded

Ware people as mobile homes (Kristiansen *et al.* 2017), but what their practical effect on subsistence practices might have been is unclear; short-range, local movement of goods and substances (perhaps such as manure) might have been their main contribution.

Animal dung could also be considered as a secondary product (Greenfield 2015, 452). Recent isotopic studies of carbonised crop remains strongly suggest that manuring was already a feature of early cultivation regimes (Bogaard *et al.* 2013; Styring *et al.* 2017). That also serves to weaken the original secondary products model. Manuring practices may have been quite diverse, and there so far appears to be little clear indication of steady intensification through time, given constraints of herd sizes (see below) and the labour involved in shifting manure in quantity over any distance (Bogaard *et al.* 2013, 12590–1). It has also been pointed out that the original secondary products model did not include any direct examination of animal bone assemblages (Marciniak 2011; Greenfield 2010; 2014). When age and sex profiles are examined in close detail, for example in the central Balkans and the Polish lowlands, there is much diversity from species to species (especially cattle and sheep/goats) and within regions, and it is hard to discern clear signatures in such data for extensive dairying regimes, but conversely neither for the exclusive use of primary products (in other words meat) (Marciniak 2011, 123–6; Greenfield 2005). There have been several suggestions, however, for increases in herd size through time in particular contexts, for example in the Vinča culture (Orton 2010) or in the Alpine foreland Neolithic sequence (Bogaard *et al.* 2017, 133).

So at the obvious risk of over-generalisation, people tended crops and animals through the many generations of the Neolithic in ways that would have seemed very familiar to both their predecessors and strangers from further afield. Most crops were probably grown in manured gardens or small fixed plots (Bogaard 2004; Bogaard *et al.* 2013; Jacomet *et al.* 2016), though shifting cultivation might have played a role from time to time and place to place (Schier 2009a). Many detailed studies show a range of crop preferences at any one time, and shifts through time (Styring *et al.* 2016; Bogaard *et al.* 2017) (Fig. 6.10). People exploited cattle, sheep and goats, and pigs, for both primary and secondary products. Animal husbandry was probably often closely integrated with cultivation (Halstead 2006), though we know rather little about the size of herds or patterns of individual and communal ownership. Preferences for some species over others changed through time, for example in the turn to cattle after the initial Neolithic in south-east Europe when sheep and goats were the

animals of choice. This could be either a response to different environments, for example as people penetrated the woodlands of central Europe (Halstead 1989) or the outcome of changing household and communal politics. Cattle figure prominently in studies of the Polish lowlands as providing meat for feasts involving more than the single household (Marciniak 2005). Isotopic studies hint at structured movement of cattle herds around local and perhaps more distant landscapes, for example in the LBK and in the Alpine foreland (Knipper 2011; Gerling *et al.* 2017). Cattle burials in Baden, TRB, Globular Amphora and other contexts (variously summarised, with older references, in Whittle 1996, [chapter 7](#); see also Pollex 1999; Bogucki 2011; Heyd and Walker 2015) may speak to the increasing importance in some contexts of cattle as a source of wealth.



Fig. 6.9: Waterlogged travois under excavation in the Chalain 19 site, from the very end of the 31st century BC, with reconstruction of possible traction arrangements. Photos: Pierre Pétrequin.

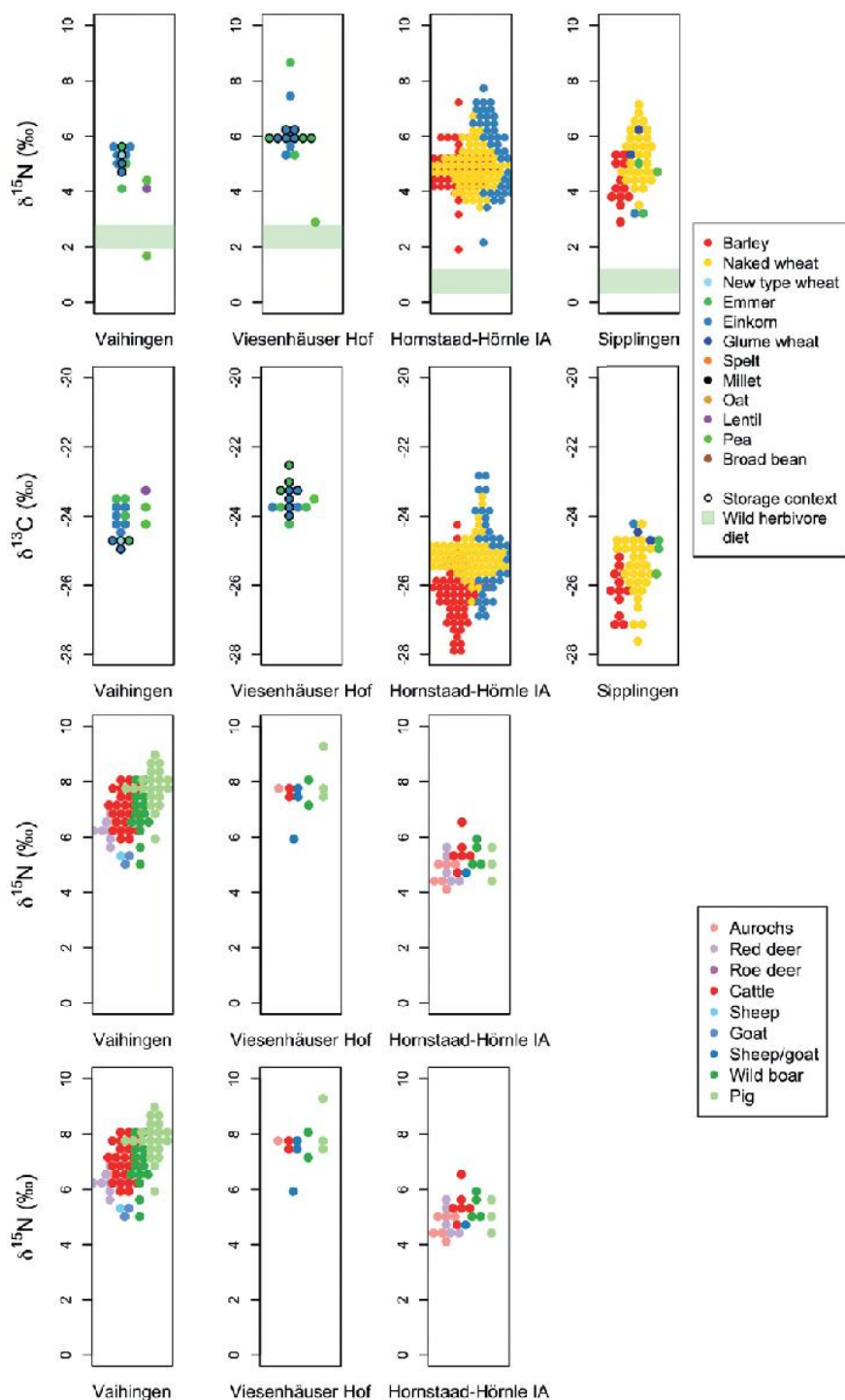


Fig. 6.10: Isotopic analyses of cereal and animal components of the Neolithic diet at LBK (Vaihingen and Stuttgart-Mühlhausen Viesenhäuser Hof) and Alpine foreland fourth millennium BC (Hornstaad and Sipplingen) sites. After Styring et al. (2017).

None of this is to argue for uniformity or lack of change in local and regional sequences, as social goals and the context of making a living were adjusted through time. It has even been argued that cultivation declined markedly in the later fourth millennium cal BC in both Ireland and Britain (Stevens and Fuller 2012; Whitehouse *et al.* 2014). But it is to try to define the overall character of subsistence practices and the tempo and nature of change in them. In this dimension of Neolithic lives, things may generally have varied less rather than more, and altered rather slowly over the centuries. For southern Europe as a whole, for example, one synthesis concluded that people ‘subsisted primarily on cereal and pulse crops, probably grown under small-scale, intensive, and stable conditions. Livestock were of secondary dietary significance, though integral to crop production and social interaction’ (Bogaard and Halstead 2015, 402). In the Alpine foreland moderate arable intensification is envisaged following the introduction or increase of animal traction in the mid-fourth millennium (Bogaard 2011, 274, 277; Bogaard *et al.* 2017); the arable weed assemblages at Arbon-Bleiche 3 do not look much different to those at Hornstaad-Hörnle 1A some 600 years earlier (Bogaard 2011, 273). Even in the early third millennium, there were short-lived settlements in the northern Alpine foreland where people were still carrying out cycles of small-scale clearance in the woodlands (Bleicher 2009a), in ways that someone who lived in the late fifth or early fourth millennium would surely have recognised. If there really was a shift to a more mobile way of life and a predominantly pastoral economy with the appearance of the Corded Ware culture in the early third millennium (Kristiansen *et al.* 2017), that would have been a profound shift from previous patterns of production, long established and long lasting, though the practice of moving animals around landscapes (e.g. Knipper 2011; Gron *et al.* 2016; Gerling *et al.* 2017; see also Seidel 2017b) would have been familiar enough.

Feet on the ground: population issues

That a more sedentary lifestyle and a robust suite of domesticated plants and animals should have brought an increase through time in population numbers, seems uncontroversial. Less residential movement, reduced birth spacings and a more secure food supply for farming communities (Larsen 2014) were presumably among the factors producing an increase in fertility over mortality. The converse situation of population decline or collapse – mortality outstripping fertility – is also to be taken into account. I am interested here above all in the tempo of such changes. This is a complex topic, more or less neglected in Neolithic studies for some decades after a burst of interest

in the days of early processualism, but now back centrally on the agenda (Bocquet-Appel and Bar-Yosef 2008; Bocquet-Appel *et al.* 2014; Shennan 2013). We need to consider it with much caution.

My main suggestion is that through the many regional sequences across Neolithic Europe there is a tendency for populations to grow gradually through time. That may seem a rather banal statement, but its implications are important. The evidence is diverse, ranging from numbers of observed sites, site sizes and durations (Denaire *et al.* 2017; Pétrequin 1996; 1997; 2005), and the density of finds within sites (Bogaard *et al.* 2017, 127), to the intensity of clearance seen in pollen diagrams (Hinz *et al.* 2012; Bunting *et al.* in prep.; Farrell *et al.* submitted) and the numbers of immature individuals in mortuary populations (Downey *et al.* 2014). Such evidence is also hard to marshal, given that the nature of the archaeological record itself is prone to vary through time, settlements changing for example from nucleated to dispersed, and so on. In the long run, even very small increases in fertility over mortality would have an effect, perhaps barely perceptible from generation to generation, but ultimately producing changed circumstances. Examples are not far to seek, such as in the Vinča network, the LBK world, the post-colonisation Italian and Iberian peninsulas, and elsewhere. One striking case is western Europe in the later fourth millennium cal BC, where against a backdrop of increased clearance and cultivation in pollen diagrams and dendrochronological analyses (Hinz *et al.* 2012; Pétrequin 1997; 2005), there was a widespread fashion for collective burials on a scale not previously witnessed. As discussed in the previous chapter, these were clearly not all the same, the *hypogées* of the Marne district being much more concentrated than the *allées sépulcrales* of the wider Paris basin (Blin 2015; Chambon *et al.* 2018), but accepting all the variety, these sorts of deposits could well reflect concerns with territorial definition, access to resources, land tenure and fertility in general. These practices appear to come to an end by the late fourth millennium, more or less coinciding with declines in clearance and cultivation indices where such evidence is available (Hinz *et al.* 2012), a development which some have even seen as a crisis (Kristiansen *et al.* 2017, 335). If that is the case, slow long-term increases could in the end have produced rapid changes.

Counter to this proposal of gradual and long-term population increase, there may be examples of rather more rapid growth. The obvious case is that of the LBK, but its initial diaspora is so rapid (Jakucs *et al.* 2016) that it is hard to see that as driven by population growth alone, even though, as discussed earlier, it coincides with the

beginning of the Vinča sequence and its own expansion of settlement. Subsequent LBK development has been modelled as reflecting rapid population growth, leading to crisis and rapid decline (Bocquet-Appel *et al.* 2014, citing climatic challenges and famine; Shennan 2013, 304, claiming for example ‘crash and local abandonment’ in the sequence of the Aldenhovener Platte). I have also discussed earlier how a compressed duration for the LBK could affect our view of such possibilities. Others of our ToTL studies have also thrown up apparent surges in activity followed by decline, as seen in mortuary activity at Valencina de la Concepción (García Sanjuán *et al.* 2018) or in settlement and monument evidence in Orkney (Bayliss *et al.* 2017). But in these latter cases, it is by no means clear that declines were due to problems with numbers of people, and more probable that social tensions are the culprit. We are also aware of how local or regional circumstance can throw people together rapidly. The strongest example of relocation on the ground comes in the study, discussed in previous chapters (especially [Chapter 4](#)), of the major aggregation at Alsónyék (Bánffy *et al.* 2016). That formed rapidly, but then declined slowly.

It is also worth reflecting on other studies of LBK settlement. While it seems clear that there were high local densities of population in favoured parts of the landscape (Zimmermann *et al.* 2009), there was genuinely more or less infinite other space to be colonised, if population numbers alone had been the major problem. However, it is not until rather later, in the later fifth millennium and subsequently, that we see the beginnings of a major intake of the interfluvies above the valley settings preferred by the LBK and its ‘Danubian’ successors. Our detailed regional study of Lower Alsace, while charting complex shifts in site numbers and locations, did not suggest an over-populous landscape by the late LBK (Denaire *et al.* 2017). So other possible factors such as societal tensions, or disease (see also Shennan *et al.* 2013, 4), have to be taken into account rather than just population figures on their own.

Into this debate has come a more radical perspective of a pattern of marked boom and bust in population numbers (Shennan 2013; Shennan *et al.* 2013). This is prefaced by a welcome challenge to the default view of slow, gradual change, and support for more dynamic Neolithic sequences in general (Shennan 2013, 300–1). That chimes well with the themes of this book. Wide-ranging ‘big data’ studies have been carried out, using large regional databases, and carrying the possibility of detecting major trends over big areas, principally so far in western Europe. That is also to be welcomed. The method relies

on the gathering in of all available radiocarbon dates, their presentation as summed probability distributions, and the use of those as proxies for rises and falls in population levels (Shennan *et al.* 2013; Downey *et al.* 2014; Timpson *et al.* 2015; see also Porčić *et al.* 2016 for a similar study of the central Balkans in the early Neolithic). The outcome, over somewhat imprecise timescales estimated in ‘100s rather than 1000s of years’ (Shennan 2013, 302), is a view of multiple regional sequences which offer growth over a period of centuries, followed by decline (Shennan *et al.* 2013, table 1). These patterns have differing timings, depending on region, even within culturally similar traditions (such as the LBK) and with estimates for periods of boom ranging from 250 years to over a millennium. There is not space here to go into them all in detail.

All three elements of the approach noted above are, however, open to question. Uncleaned databases risk introducing bad dates and noise; as discussed earlier, summed probability distributions offer little precision and may simply reproduce the patterns of the calibration curve; and the assumptions behind the claim that the intensity of radiocarbon dates can mirror population levels are also open to serious question (Torfing 2015a; 2015b). I would not dismiss the resulting ‘big picture’ out of hand, but the proof of the pudding here lies in careful reflection on where this method overlaps with the Bayesian approach advocated in this book. Two comparisons can be made. The complex sequences for southern England (‘Wessex-Sussex’), Ireland and Scotland are reduced to three lines in a table (Shennan *et al.* 2013, table 1, fig. 3), in stark contrast to the detailed treatment offered in Whittle *et al.* (2011). The two approaches also offer radically different perspectives on the spread of change at the start of the Neolithic in Britain (Collard *et al.* 2010; Whittle *et al.* 2011); they cannot both be right. For Rhineland-Hesse, a pattern of continuity is identified from the mid-sixth to the mid-fifth millennium cal BC (Shennan *et al.* 2013, table 1), which is at odds with the discontinuity at the end of the LBK identified in our ToTL study of the Rhineland as a whole in comparison to Lower Alsace (Denaire *et al.* 2017). The danger is that the complexities of regional sequences are being flattened by the summing, proxy approach. [Chapter 4](#) has already set out a more detailed and rigorous way of trying to derive population estimates from the precise chronological sequence of an individual site (and see again [Figs 4.12–4.14](#)). That could be reproduced in many other situations, though this will be slow and hard work.

Lifetimes and generations

The two scales which I have examined so far in this chapter could

imply a view of Neolithic history in which, after and behind the eventful and varied spread of the new way of life, very slow change over the long run, barely perceptible in the flow of daily life, the seasonal and annual repetitions of making a living through tending crops and animals, and the constant balancing of births and deaths, was punctuated at quite big intervals by times of major change. This would not be a history in which nothing happened, and even major disruptions such as the end of tells and the appearances of Corded Ware and then Bell Beakers were successfully identified by culture historical approaches, but it is one in which slow change and continuity tend to be the normal, default perspectives, reinforced, when aided by the visual inspection of radiocarbon dates, by the definition of cultural phases as generally no shorter than a couple of centuries.

Throughout this book, however, I have tried to show how the ability now available to create more precise chronology adds vitally important further perspectives. Mostly that is through the formal modelling of radiocarbon dates, though for the Alpine foreland and a few other, rather isolated cases, dendrochronology is possible. Dendrochronology in the Alpine foreland currently offers the most precise temporal records available for study of the European Neolithic, and that will presumably remain the case. From it comes a detailed view of house and village durations, local histories of clearance and woodland management, and the pulse and tempo of cultural change, in a perhaps surprising mix of things, some lasting for only very short periods of time but others enduring for much longer. Formal modelling of radiocarbon dates cannot yet match this kind of discrimination, but it is getting much closer. I have argued over and over again in this book, drawing on a wide series of case studies – over a range of subjects including the duration of houses and settlements, the pattern of settlement across landscapes, the character of burials and monuments, and the regional sequences of material culture – that if we do the right things we can successfully and routinely get to the scale of lifetimes, generations and even decades. We can even outwit, as it were, when all goes well, the vagaries of the calibration curve. All this gives not only better and more reliable timings but also a sense of varying duration and tempo, and importantly, from time to time instances of abrupt change and even hiatus.

I present one last case study, that of the biography of the Uivar tell in western Romania, to reinforce this ambitious claim one more time, and then beyond that, I want to consider how we now bring all these

different scales together to construct dynamic and complex Neolithic histories, which while not identical can match in their own way the narratives of historians.

The biography of the Uivar tell

Just one of the tens of such sites in the northern distribution of tells, Uivar lies in the Banat region of western Romania, near the Brega river, a tributary of the Tisza (Schier 2008; Draşovean and Schier 2010). Covering some 3 ha and 4 m high, the tell was clearly locally significant but might be thought of as otherwise unremarkable in the wider context of tells across south-east Europe as a whole (Anthony 2010); this was not a settlement mound of the magnitude of Vinča-Belo Brdo. Demonstrably it had a long history, identifiable from the height of the stratified deposits, the first radiocarbon dates, and the range of pottery styles uncovered by the excavations from 1999–2009; those extended from the Szakálhát to the Vinča C1 and C2 styles, for example (Schier 2008, 61; 2009b; Draşovean 2009). The history of the tell could simply be folded into a longer and broader narrative about the occupation of this part of the Carpathian basin, with the nucleation represented by the mound following more dispersed settlement in the Criş and subsequent regional phases (Schier 2008, 61), and in turn succeeded by the again dispersed conditions of the Early Copper Age (Draşovean 2009; 2014). This is a story which resonates with other general accounts of long-term trends in the wider region in which Uivar lay (Sherratt 1982; Tálas 1987; Borić 2015a), its headline being a continuity of occupation of place maintained over a span of centuries. Other parts of the story reside in the details of the changing techniques of house building through successive layers (Schier 2008, 56–60; Draşovean and Schier 2010; Draşovean *et al.* 2017) and of the agricultural economy which the inhabitants of the tell practised, evidenced by rich finds of animal bones and cereal and other plant remains (Schier 2008, 65).

With more radiocarbon dates and formal modelling, however, a much more dynamic biography has become apparent (Draşovean *et al.* 2017; Schier *et al.* forthcoming). Site stratigraphy, material culture, radiocarbon dates on short-life, single-entity samples and formal Bayesian modelling provide a powerful framework for a refined chronology of the Uivar tell, which I have already noted several times in earlier chapters (Figs 6.11–6.14). The chronological model for Neolithic occupation at Uivar combines a total of 182 radiocarbon dates with the archaeological sequence, crucially with the 4 m of vertical stratigraphy through a series of 11 building stages through the tell excavated in Trench I (Draşovean *et al.* 2017; Schier *et al.*

forthcoming). It is important to note that this was a trench of modest proportions, and with post-excavation analysis still continuing it is difficult to link it with finds in other cuttings.

The tell was occupied from the 52nd to the 47th centuries cal BC (Draşovean *et al.* 2017). Its history can be tracked in the estimates for both successive occupation levels and house building stages, and the various encircling ditches revealed by geophysical survey and excavation, some tight in to the foot of the tell but the largest forming an egg-shaped enclosure around the mound, over 350 by 200 m in extent and defining a total area of some 12 ha. This is a story at its most powerful when the two elements are combined.

So, from the small scale of the excavations on the mound, we do not know what the initial layout of the site was like, but identified buildings in the relevant trenches appear to have been set regularly and close together. The first buildings were very solidly built, and the ceramic connections of the site at this stage were with the Szakálhát group or network centred in southern Hungary (Kalicz and Makkay 1977). Through its sequence, some house durations were shorter (in building stages 5b, 5a, and 3a), and others longer (in building stages 4b and 4a), the spans of use ranging from a decade or so to up to some 50 years. Building stages 3d and 3c had to be combined as a single value in the modelling (no dateable material could be located from stage 3c), but its total duration can be broken down into two, given the evidence for extensive repairs through the thick level in question rather than the more usual levelling and rebuilding. Building stage 2b is the longest-lasting, with a probable duration of some 80 years. There is an apparently cyclical pattern of alternating shorter and longer durations, from the shorter lives of the early houses in stages 5b and 5a to the longer biographies of the houses in stages 4b and 4a, from the two phases of stage 3d + c to the longer existence of stage 3b, followed in turn by the briefer duration of stage 3a and finally the longest span of all, building stage 2b (Draşovean *et al.* 2017). The uppermost Late Neolithic building stages 2a and 1f consist of some foundation structures, but are heavily disturbed by medieval pits and have provided little dating evidence (Draşovean *et al.* 2017).

On the basis of ethnographic analogy, we have speculated whether the duration of the initial buildings might have been deliberately truncated, to create renown and ancestry for the site (Draşovean *et al.* 2017). There were also what seem to be dramatic events at the end of building stages 4b and 4a, each having signs of the burning of more than one building. Were these just accidental fires, with possibly extensive effects, something akin to the fires that still plagued towns

and villages in 19th-century Europe (R. Evans 2016, 371–4), or could they be a sign of deliberate aggression by others? Now ditches appear to have encircled the settlement from the beginning or soon after, and the excavators confidently interpret them, from their substantial width and depth, as protective and defensive. Ditch F1237 seems to have been the defensive system that was there at the end of building stage 4b, and the combination of its demise and the possibly extensive burning at the end of building stage 4b could suggest a violent interruption to the life of the site, already by the 51st century cal BC (Draşovean *et al.* 2017). The same may apply with the end of building stage 4a and ditches F1236=F1238 and F1219, only a couple of generations later (Draşovean *et al.* 2017). That could imply the continuation of troubled times, but also speedy rebuilding and resumption.



Fig. 6.11: Top: the Uivar tell; below: reconstruction of its possible appearance c. 4800 cal BC. Photo: Wolfram Schier; drawing: Stefan Suhrbier.

Next probably came ditches F1055 and F1158, which probably go with building stages 3d+c or 3b. In these instances, there is no question of relating the demise of ditch circuits to house burnings, since none were observed in the excavated parts of the tell in those stages. On the line of argument followed here (the alternative being that ditches were simply in various ways symbolic, to mark and enhance the limits of the site), this could indicate a period of more

settled times, which would correlate with the appearance of first Vinča C1 and then Vinča C2 pottery, in the 50th or earliest 49th centuries cal BC, as the connections of the site shifted to Serbia and the south, at the time of the greatest expansion of the Vinča network as a whole (Whittle *et al.* 2016).

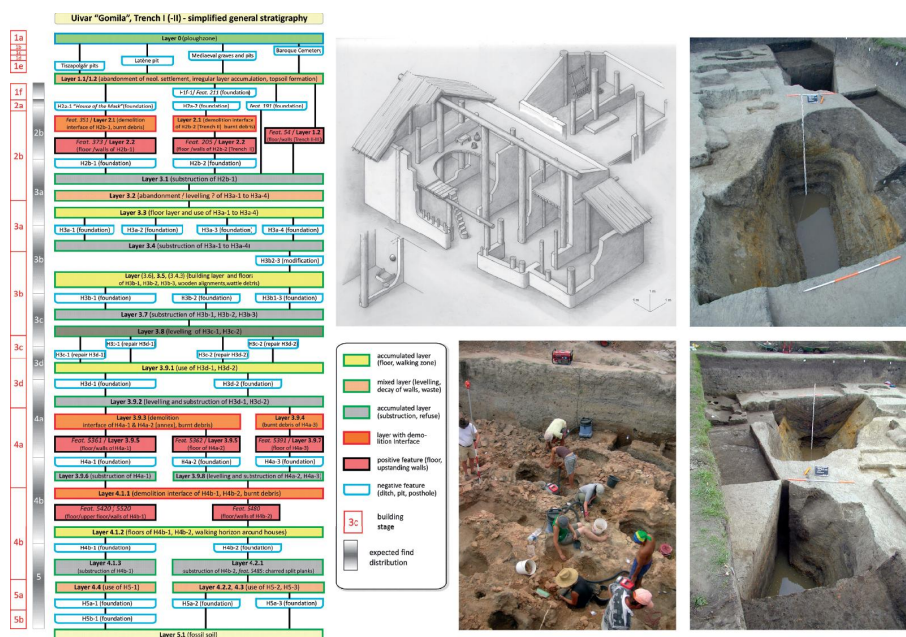


Fig. 6.12: House 4b and its reconstruction, and the inner ditch at Uivar under excavation, and the resulting working Harris matrix. Photos and matrix: Wolfram Schier; reconstruction: P. Kunz, Institut für Prähistorische Archäologie, Freie Universität Berlin.

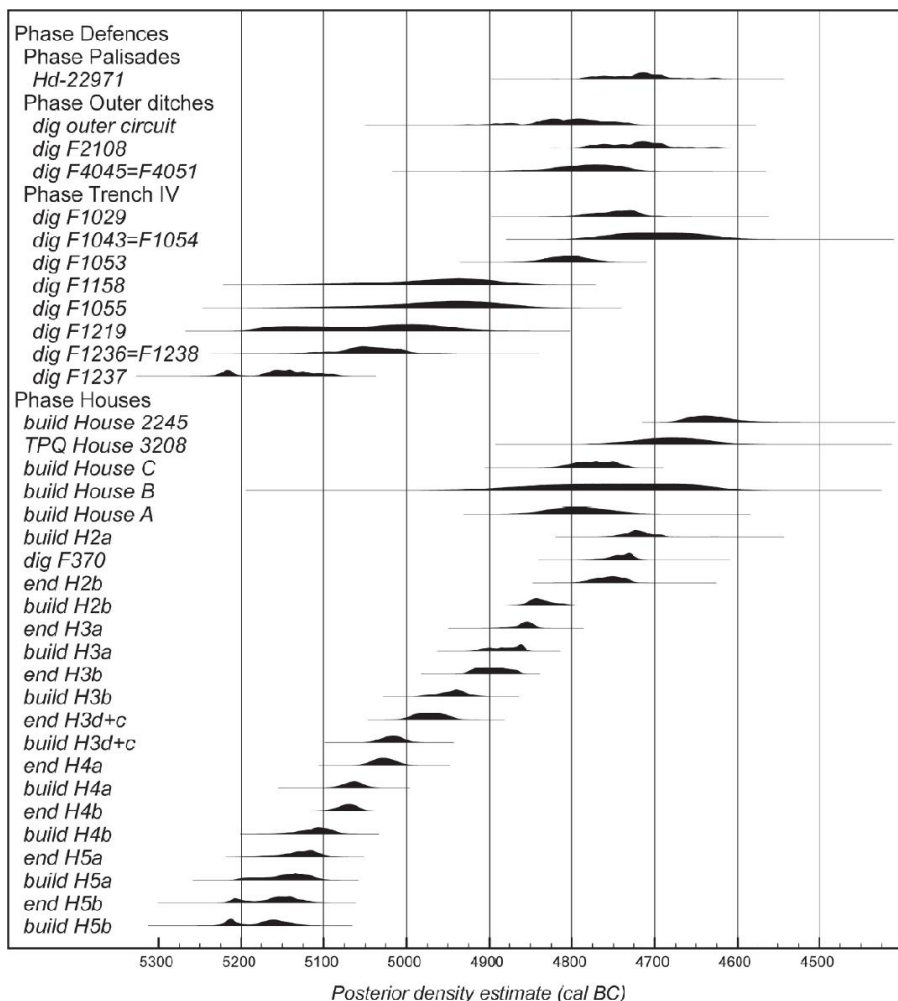


Fig. 6.13: Model for the sequence of houses through the tell sequence at Uivar, and of ditches surrounding the tell. After Draşovean et al. (2017).

Then the outer circuit, ditch F1053 and ditch F4045 = F4051 were dug, all probably going with building stage 2b. If burnings and the end of defensive circuits can be linked, then these were the ditches that could have failed at the end of the longlasting building stage 2b, which also saw extensive burning, this time supported by the evidence of geophysical survey for numerous burnt anomalies right over the area of the top of the tell. The end of building stage 2b probably fell in the 48th century cal BC. This was not quite the end of it all, since some further occupation on the top of the tell took place subsequently and in the area immediately surrounding the tell. The most impressive of all the Uivar ditches, F1043 = F1054, probably marks the rebuilding of the defences following this 2b fire, joined by ditch F2108 (and a palisade in Trench IX). This was probably in the later 48th or earlier

47th century cal BC. After this, occupation seems to have ended, resuming only in sporadic fashion in the Tiszapolgár phase some centuries later (Schier 2008, 61).

So within the life of the tell, alongside long-term continuity, gradual shifts in building techniques and varying house durations, there were three probably major burnings, plausibly (but not uncontroversially) connected with violent attacks by others, repeated redefinition of arguably defensive circuits, and one spell of more settled times, coinciding with the maximum extension of the Vinča network. This is a much busier and more varied biography than given in models which simply stress continuity of place above all else. This is not to deny such continuity, but rather to put it now into more detailed context, as something achieved against a dynamic setting of changing allegiances, connections and relations with neighbours and others. This is presumably also a story which does not and cannot stand on its own, though our knowledge of the detailed chronology of neighbouring sites is so far somewhat imprecise. Another locally significant site, for example, is Parța, only 18 km away in the Timiș valley, whose occupation may have started a little earlier than Uivar and overlapped with it for a few generations (Drașovean and Schier 2010; Drașovean 2014; more details in Schier *et al.* forthcoming). Was this site, some of whose buildings were larger than others and heavily invested with ritualised settings and features, a rival community, possibly with more internal social differentiation, and was it the deterioration of relations between the two which produced burnings at both and a premature ending for Parța?

It will be a measure of future success if our understanding of this and other contexts is radically expanded. Instead of currently isolated sequences from region to region, for example at Csőszhalom (Raczky *et al.* 2015), Alsónyék (Bánffy *et al.* 2016) or Okolište (R. Hofmann 2013), imagine what it would be like if detailed accounts of neighbouring sites, such as Berettyóújfalú-Herpály (Kalicz *et al.* 2010) or Zengővárkony and Mórágý-Tűzkődomb (Zalai-Gaál 2010; Regenyé *et al.* in prep.), were readily available.

The formal modelling of Uivar presents a timed ending as well as a more nuanced unfolding of the story of its long occupation. Without further formal modelling it is unclear whether it was immediately succeeded by more dispersed settlement, or whether some kind of local gap or hiatus ensued before things picked up again, in different form. I have stressed other studies which do suggest hiatus, notably but not only at the end of the LBK. That is another important counter to the default expectation of slow change and continuity, and it is one

which has only become more apparent, apart from through the impact of recent genetic studies, from chronological studies that reach the precision of lifetimes and generations.

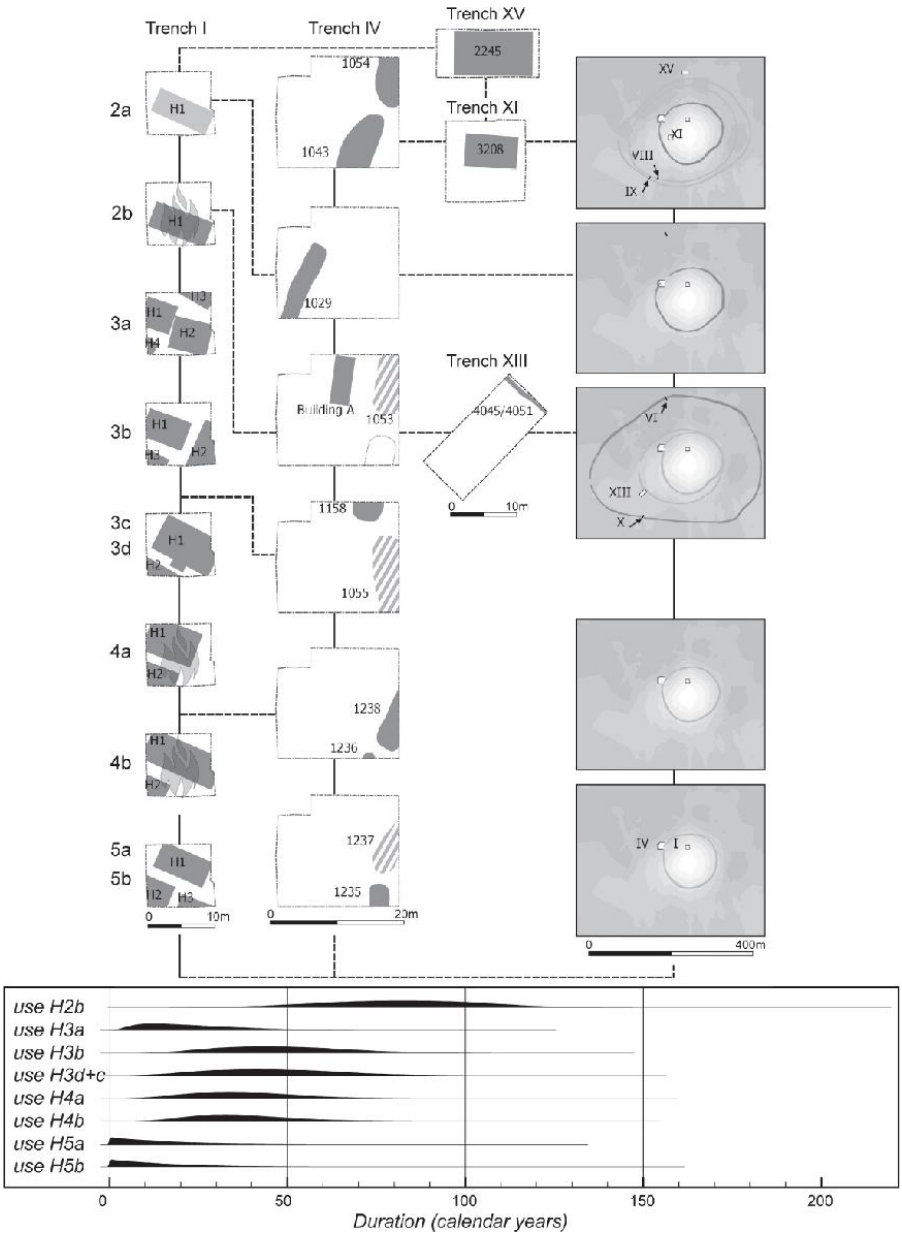


Fig. 6.14: Uivar building phases: top: outline interpretation of the sequence; below: modelled house durations. After Dragovean et al. (2017), and see Schier et al. (forthcoming).

Up and down the scales

There is much more to weave into Neolithic narratives than we often

give credit for, especially if intimidated by the distance of the past and the lack of written records. In this chapter, I have tried to show how attention to possible frameworks outlined by major congruences, turning points and disruptions and even hiatus can be combined with a sense of slower time playing out in the endlessly repeating rhythms of daily life, the demands and routines of making a living through subsistence practices, and the probably slow but inexorable growth of population. These are best illumined, however, when we can get to a greater chronological precision, that of lifetimes and generations, which allows us to order and dissect sequences in much greater detail.

That does not do away with the need or the desire to form the big picture, but it helps to avoid simply bundling all evidence indiscriminately into large, unsorted blocks. Narratives everywhere can be improved by better estimates of timing. When we started the ToTL project, we believed that our main contribution would be to help to refine aspects of the edifice of Neolithic chronology built up by over a century of research, and we hope that we have indeed fulfilled that aim. But the most successful outcomes also reveal the weaknesses of that existing structure and the default expectations to do with the character and rate of change associated with it. Better timings enable a more precise sense of duration, and sufficiently numerous results allow comparison of tempo. There is still an enormous amount of research of this kind to be carried out, but already we can point to the significance of particular centuries, in a way previously barely possible. In this book, I have highlighted particularly the 54th, 46th (and 47th), 37th, 28th and 25th centuries cal BC, and further research will surely turn the spotlight on others. I do not believe that there is something essential or profound about a century; four generations may be a better way of thinking of that span. But it gives a measure of the control which we can now exercise over the Neolithic record in pursuit of narrative.

By narrative, I repeat that I do not mean mere chronicle. Through narrative, we should seek to interpret the evidence. My accounts have been focused on human action or agency, intentionality, values, networks of connection, social relations and a rich web of culture. In [Chapter 5](#), for example, I suggested that there was a long-running tension between communal values and the tendency of some individuals and groups to aggrandise themselves; that pursuit of enhanced position was often successful, but never, in my view, for very long. This serves to define one important dynamic in the Neolithic narrative, outlining not a story of nothing significant happening for three millennia or more but an often subtle and

complex set of cycles of development and change. There is much more to Neolithic history than the initial establishment of new people, things and practices – important though that process is – and we can now begin to present it in ways that bear comparison to other forms of writing history.

That writing involves negotiating varying scales, and I have tried to show how many of these are now available for detailed inspection; once again, the ability to get to the scale of lifetimes and generations, even decades, is of paramount importance. When we started the ToTL project, I thought that perhaps with further research and reflection we could discover some kind of magical alchemist's formula for combining multiple scales in a single or universal approach. That, of course, is an illusion. What we need is as much precision as we can assemble, and then armed with that, we have to confront the challenge of constructing narratives at varying, sometimes multiple, scales to best suit the demands of our chosen kinds of story-telling. I have claimed in this book that we can do that now in ways vastly superior to what was previously possible. But we should not be satisfied with the current situation, and in the last chapter, I attempt to peer ahead to glimpse what could be done even better in the future.

Notes

1 Obituary in *The Times*, 24 September 2015.

2 As can be seen in the Bibliography, the paperback subtitle is 'How a remarkable woman crossed seas and empires to become part of world history'. The original subtitle was the more sober 'A woman in world history'. As Colley drily comments (*British Academy Review* summer 2016), 'Hey, I'm only the writer'.

3 For a very readable and intelligible introduction, see Rutherford (2016) and see now (added in proof) Reich (2018).

4 Based on mitochondrial aDNA; the figure is probably too low, and 5 percent could be more appropriate: Iain Mathieson, *pers. comm.* For Britain, see now also (added in proof) Brace *et al.* (2018).

5 A point I owe to David Reich and Iain Mathieson.

6 Also now given as Southern Buh and Dniro, in transliteration of the Ukrainian rather than Russian forms.

7 And perhaps fermented curds and whey: Eszter Bánffy, *pers. comm.*

8 See also the Berlin Topoi project, *The Textile Revolution Research Group A-4*: www.topoi.org/group/a-4/; C. Becker *et al.* (2016).

Chapter 7

Kinds of history: the future of the Neolithic past

My argument: narrative and history for the remote Neolithic past

I began this book by stressing the importance of narrative. I have argued throughout that we are now in a position to release the people of the Neolithic past in Europe, remote from us by several thousand years, from the all too often chronologically very imprecise and interpretively generalised narratives which we have constructed for them. Instead, we can, if we do the right things with choosing the best samples for radiocarbon dating and then formally modelling the results, achieve much more robust and much more precise chronologies of their lives, down to the scales of lifetimes and generations, and even on occasion to the scale of decades. From such chronologies, we can construct much more detailed narratives for the actions, choices and decisions of people in the Neolithic, returning those to centre-stage. Narrative requires interpretation, and so goes far beyond mere chronicle. Narrative is therefore both personal and provisional, and other scholars will tell the many Neolithic stories differently from my ways of attempting to do so. My account has sought to work at multiple scales. I do not believe, however, that there is any magical, single or pre-given combination of scales. I have argued that the most illuminating scale is the one at which we can begin to investigate people's actions over lifetimes and generations, because that both serves to unpick the long term and to place what may seem like the endless and repeating flow of daily existence into specific and changing contexts, but I do not thereby want to abandon interest in such other scales. I have claimed that as a result we can shift from a previous default perspective of change as more often slow than fast, with much continuity from phase to phase or horizon to horizon, to a new view of an often much more dynamic, rapidly altering and punctuated Neolithic past; that is compatible with the renewed demonstration, from the welter of recent aDNA studies, that we have to do with substantial movement of people at significant points in the history of the Neolithic, particularly at what I have defined as the beginning and the end. I have set out why I think the construction of

such narratives constitutes now a kind of writing of history, even though we have no written records at our disposal for the study of the Neolithic in Europe, and how concerning ourselves much more with particular situations and contexts, and ridding ourselves of a deeply rooted addiction to and reliance on generalisation, would be very healthy for the discipline of archaeology as a whole. The pre- must come out of prehistory.

For the reader who might be browsing the start and end of this book before plunging into its detail, I have set out issues of scales and past approaches to chronology, interpretation and narrative in [Chapter 2](#) and the beginning of [Chapter 3](#). [Chapter 3](#) laid out what we can achieve with dendrochronology, typology and seriation, and the formal modelling of radiocarbon dates in a Bayesian statistical framework. [Chapter 4](#) provided a series of detailed case studies of the Bayesian approach in action, across a number of selected themes in the Neolithic of Europe, principally figuring settlement, monumentality and materiality; my view of history is very much centred on the actions of people. Why this matters I then addressed in [Chapters 5](#) and [6](#). [Chapter 5](#) considered the many challenges of interpreting the shifting social map of Neolithic Europe, and I argue above all that detailed studies, with the greater chronological precision now available, suggest that social differentiation was never established in any given context for long. I believe that that offers a fundamentally important insight into the nature of social relations and the trajectory and tempo of change through the European Neolithic. In [Chapter 6](#), finally, I engaged with a wider perspective, looking (all too briefly) at a possible framework of major transitions or tipping points and at dimensions of daily life, subsistence practice and population which I see as changing much more slowly over the millennia, alongside a final major example (that of the Uivar tell) of investigation of the lifetime-generational scale. As stressed, others will read such evidence differently, but it is from such sources, enhanced by much greater chronological precision, that we can craft the outlines of Neolithic history.

The future ain't what it used to be

Is that it? Far from offering a report on an accomplished task, I prefer to think of this book as part of a manifesto for future action. That involves looking ahead. Doing so is notoriously difficult, and most of the future worlds constructed by, for example, H.G. Wells (*The Time Machine* and *The War of the Worlds*), George Orwell (1984) or Philip K. Dick (*Do Androids Dream of Electric Sheep?*) are either set so far away in time to come that they remain fantastical, or prove not to be true

when their supposed moment catches up with them. Nonetheless, I think that some future-scoping is essential, and it helps to complete my argument about constructing richly textured histories for the Neolithic and other distant pasts. The journalist Matt Ridley has drawn attention to our tendency to expect too much of innovations in their first ten years, and too little after twenty.¹ But I think that there are many other exciting strands in current research which, if united with tightly constructed chronologies and narrative frameworks, could constitute a new kind of archaeology. Much of what I want to highlight is methodological, but underpinning the widening range of scientific and other analyses must be a continuing concern for what I see as the central subject of our studies: the people of the past.

It seems reasonable to predict further steady advances in Bayesian chronological modelling itself (Bayliss *et al.* 2016; Buck and Juarez 2017, 22–4), in the refinement of the internationally agreed calibration curve (Alex Bayliss, *pers. comm.*), and in the processing of big data (Bronk Ramsey 2017), as well as continuing reflection on how to integrate existing bodies of knowledge and new models (Griffiths 2017). Looking further ahead, the ongoing development of quantum computing (Trabesinger 2017; Harrow and Montanaro 2017) could potentially revolutionise the speed of processing and therefore the range of model calculation. On a more prosaic and immediate level, there is no shortage of future targets for dating and modelling; I have referred to many contexts in which the modelling achieved so far can only be regarded as the first step in a long process of narrative building. To that cause could be added further attention to both typology and seriation – in some research traditions representing essentially continuity of past practice but in others constituting a real return to the study of the material itself – and Bayesian-based typologies and seriations could be envisaged too. In such a field, the development of robust and precise compound-specific dating of pottery is potentially of enormous importance (Casanova *et al.* 2017a; 2017b), as it would serve to unite study of material change and chronology building even more closely, and to generate many more potential samples in contexts otherwise lacking suitable organic material for dating.

There are also many other approaches to combine within a refined chronological framework. The continuation and predictable future acceleration of aDNA research, with the focus now on the whole genome, will surely remove – though with perhaps still unpredictable outcomes – current reservations about the scale and geographical range of sampling.² One could predict not only a deepening of

understanding of populations but also of close individual relationships in tightly defined contexts and sequences, most promisingly in my view within cemeteries or other mortuary contexts. I have referred several times in the previous chapters to the possibility that disease could have played a bigger part in change and disruption than has been credited in the past, and aDNA research must have a crucial role in further such investigations. Progress in understanding plague, *Yersinia pestis*, is a case in point, with the latest results indicating a steppe origin, which ties in with a narrative of significant population movement associated with the Yamnaya and Corded Ware cultures (Rasmussen *et al.* 2015; Andrades Valtueña *et al.* 2017). As I discussed in previous chapters, it is to be hoped that diseases can now be tracked in earlier millennia as well.

There is already a formidable battery of isotopic analyses which provide insight into diet and lifetime mobility, from carbon and nitrogen values (and now associated Bayesian modelling: Fernandes *et al.* 2014) to those of strontium, oxygen, sulphur and lead. These can be applied to animal as well as human populations. The most ambitious investigations can achieve a range of temporal scales, from the defined early life snapshots, depending on the tissue used (teeth, femora or ribs, for example), given by strontium analysis which can provide insight into lifetime patterns of movement (or otherwise), to the seasonal scales made possible by multiple sampling of the dentine and enamel increments in animal teeth (Knipper 2011; Balasse *et al.* 2012; Gerling *et al.* 2017; Beaumont and Montgomery 2016). Advances continue; it is now possible, for example, to obtain good results from strontium analysis of calcined bone (Snoeck *et al.* 2015). There are plenty of reservations still to be made, however, neatly illustrating what Matt Ridley notes as Amara's Law (our tendency to overestimate the impact of new technology in the short run but to underestimate it in the long run);³ there is a potential danger of this pattern occurring in the reception of isotopic analysis (e.g. Bickle and Hofmann 2007; A. Pollard 2011). Thus there is much still to do in establishing robust baseline maps and a better understanding of diagenesis for strontium analysis (J. Evans *et al.* 2012; Madgwick *et al.* 2012) and in developing optimal approaches to the data for oxygen analysis (A. Pollard *et al.* 2011; Lightfoot and O'Connell 2016; Pellegrini *et al.* 2016). Conventional bulk carbon and nitrogen analyses can be seen as a relatively blunt (though still very effective) instrument, compared to more refined compound-specific approaches, which are still under development (Evershed 2007; Webb *et al.* 2015; Britton 2017, 856, and further references). But the optimistic view

must be that the combination of developing multiple approaches and large datasets will continue to open new insights into not only diet but also other aspects of nutritional stress and disease (Britton 2017, 856; Reitsema 2013).

The latest and very exciting kid on the block is the dental microbiome. The realisation that dental plaque or calculus can trap past traces of food and environment, and indicate the state of oral hygiene and health, as well as providing samples for aDNA analysis, has potentially revolutionary implications (Warriner *et al.* 2014a; 2014b; 2015a; 2015b; 2017; Radini *et al.* 2017). Starch granules (from roots, tubers and seeds) and plant phytoliths, pollen, micro-charcoal and plant fibres are among the trapped residues of diet and environment (Warriner *et al.* 2015a, 3–4), while genomic analysis allows insight into ancient oral health (Warriner *et al.* 2014a); it has even been possible to detect the first direct evidence of milk consumption, the whey protein β -lactoglobulin (BLG), preserved in human dental calculus from Bronze Age samples in Russia and Hungary (Warriner *et al.* 2014b). Not for nothing is this heralded as a ‘new era in palaeomicrobiology’ (Warriner *et al.* 2015a, 7). As sampling is extended, not only many more individuals but populations could be characterised, with genomic research linked and integral to investigations as well. The timescale over which diet, health and environment are thus recorded is presumably a lifetime one, since residues accrue ‘incrementally through serial deposition and mineralization in situ ... making it a layered record of human life history specific to each person’ (Warriner *et al.* 2015b, 128). Whether this can be exploited on a fine scale within individual lifetimes (Radini *et al.* 2017, 73), and whether the entrapment of residues in calculus is systematic enough to give a balanced record of individual diet as a whole (Radini *et al.* 2017, 74), remains to be seen. I have concentrated here on recent and eye-catching applications of biomolecular analysis, but microscopic and conventional macroscopic analyses should not be neglected, and add further scales and potentially powerful combinations. Studies of dental microwear, for example, should give information about diet and health in the last weeks of life (among many others, see Forshaw 2014; El Zataari and Ungar 2017; Scott and Halcrow 2017).

These and other examples should serve neatly to demonstrate the ending of the ‘science wars’ – the clash between modernist advocates of science and post-modern critics, as John Robb (2014, 22–3; see also Bickle and Whittle 2014) has put it – of a previous generation of research, though to judge by some of the initial reception of aDNA

results in the recent literature there is still plenty of skirmishing going on. In archaeology, it has been proposed that previous divisions between processualism and post-processualism can be settled by a new vocabulary including materially embedded sociality, ethnographies of daily life, genealogies of practice, and so on, with themes such as foodways and cuisine, lived landscapes, and inhabitation to the fore (Robb 2014, 27–31). My vision for future early history – to replace prehistory – would thus be investigation of the lives and sociality of people at multiple scales, harnessing the best of archaeological science. This will be all the more effective if framed by high-resolution narratives; interpretive narratives will in turn generate the questions to fuel continuing research. Imagine what might be achieved if all the techniques sketched above were combined in studies held within a robust and tight chronological framework. What would be the dream scenarios? What a wonderful start could be made with an LBK cemetery, say (or any other favourite and favourable context with human remains in the right condition of preservation), with sequence outlined by horizontal stratigraphy and seriation of decorated pottery, and refined by multiple radiocarbon dates modelled in a Bayesian statistical framework, and diet, health, lifetime movements, genetic ancestry and personal genetic relationships all opened by the now available suite of isotopic, other analytical and aDNA techniques noted above. Suppose then that the discipline could mobilise enough resources to repeat such detailed individual and collective biographies, rooted in particular times and places, across the social map of Neolithic Europe, or at the least across selected regions, and we can peer far enough ahead to make out the shape of a radically different archaeology to come. As Yogi Berra is said to have put it, with delicious if unintended ambiguity, ‘the future ain’t what it used to be’.⁴

There has been speculation about whether archaeology will at some point run through its finite resource of remains, bringing an end to discovery; case studies from the Americas and Europe are taken to indicate a decline in discovery and even the near-depletion of some of the archaeological record (Surovell *et al.* 2017). This may be true in some cases, and is not to be taken lightly, but it is rather like knowing that one day the sun will come to an end and finish life on earth; it is what we do meantime that matters. In the short term, the pace and range of discovery often appear to be increasing, as seen in many of the examples given in this book, as a result of infrastructural development or research for its own sake. I was once told by a senior academic with a hand on the nature of funding at the time that

archaeological fieldwork was a cottage industry, full of activity but serving little wider purpose. It has also been argued with reference to both history and archaeology that what society needs are answers to the big questions, over longer rather than shorter timescales (Guldi and Armitage 2014, 124–5; Robb and Pauketat 2013, 31–3). There is no reason at all to give up such big questions of human development and social change across time, but I think it would be a dangerous strategy for our discipline(s) to define all research in these terms. Following that perspective, it would be all too easy for supposedly smaller questions to be ignored, with research underfunded or withdrawn. Specific, local contexts can matter just as much as grander themes, and it is often that sense of the particular which most attracts people (including students) to our subject in the present day.

So I see no shortage of things to do, for the foreseeable future. What, finally, are the threats and obstacles to constructing much more precise, contextualised and textured narratives of the Neolithic past? Of course, there must be concern about the scale and nature of funding. In Britain at least, most fieldwork discoveries come from developer-funded projects within the state planning framework (Barker *et al.* 2017, 42), but such activity in the commercial sector does not always lead to prompt or full publication (Bradley 2007) or to novel analysis. Much of the impetus to analytical innovation comes from the universities, and for those, from a British point of view in a post-Brexit world, there are real threats to both national and European funding (as well as worries about the numbers of students). Nowhere do the dating and analytical techniques discussed above come cheaply, so that my vision of detailed histories covering the whole continent could take a long time to materialise, even if the discipline as a whole were committed to the same, shared goal. Nonetheless, significant progress can be made gradually and cumulatively, context by context, project by project, if enough colleagues do the right things.

In my view, the greater threat operates at a more interpretive and philosophical level, in the debate about the nature of history. That for me rests not in the end in the question of scales, important though that is, but in the matter of agency. If we can build precise chronologies nearly everywhere, what kind of narratives are we to construct on their basis? As I have discussed in previous chapters, for me the narratives are *in the end* about people and their actions, choices and decisions. I see people framed in dense webs of tradition, practice, values, culture and belief, and how agency or the ability to act in the world comes about may always be hedged or framed by

such considerations, and presumably not always in the same way. That view runs counter to the increasingly fashionable arguments for much more distributed agency, shared between many or all constituents of the world, including animals and objects, and for a much less patterned, and much more indeterminate or contingent kind of history than I have in mind. That rival perspective has been summarised neatly as an increasing shift ‘from an emphasis on direction and progress towards an acknowledgement that any form of life is held in temporary tension between a whole series of forces, which derive from past states, but not in any easily predictable manner’ (Barker *et al.* 2017, 10). As I have stressed in previous chapters, it is important not to lump all the many strands of the ontological or post-humanist turn together (Harris and Cipolla 2017). I am personally much more sympathetic to the possibility of extensive belief in animate agents, whether human or non-human, because that still centres debate in people, than to the agency – in the same sense that it could apply to people – of objects in and of themselves; equally, I recognise the danger of analogy and of constructing what have been called universalising or globalising ontologies (Alberti and Marshall 2009, 345). Likewise, narrative in the sense I have used it in this book is precisely the discernment of pattern, or of what Paul Ricoeur (1984) called plot, in the kaleidoscope of life and its many processes, and is particularly well suited to the lifetime and generational scales advocated throughout this book. Rather than chronicles of contingency, I would like to offer narratives of action, even though, as I argued particularly in [Chapter 5](#), their overall shape can be cyclical and complicated, and things can stay the same as well as change rapidly, as I argued in [Chapter 6](#). As Paul Ricoeur (1984, 56) put it, ‘in the final analysis, narratives have acting and suffering as their theme’, which I see as the fate of people, but not of objects. The hunt is on for the most detailed histories of these past people that we can achieve.

Notes

- 1 ‘Don’t write off the next big thing too soon’: *The Times*, November 6, 2017.
- 2 Though this is also bringing problems of access to samples (Makarewicz *et al.* 2017).
- 3 See endnote 1.
- 4 See endnote 1 for Chapter 6.

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